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THE PSYCHOLOGICAL REVIEW

MEDIATED GENERALIZATION AND THE INTERPRETATION OF VERBAL BEHAVIOR: I. PROLEGOMENA[1,2]

BY CHARLES N. COFER AND JOHN P. FOLEY, JR.

The George Washington University

Since Watson's epoch-making hypothesis relating thinking to sub-vocal speech, psychologists have emphasized, to a greater or lesser degree, the important rôle which language mechanisms (verbal, manual, postural, etc.) play in complex human behavior. Although there has been much speculation regarding the nature and function of linguistic behavior, relatively few experimental data are available as the basis for systematized theory in this area. In fact, language mechanisms have often been regarded as 'uncontrolled variables' in psychological investigations.

The present paper reports an attempt (1) to extend the objective principles derived from conditioned response experimentation to certain aspects of linguistic behavior, and (2) to show how such an interpretation of language behavior throws light on other psychological phenomena in which language plays an important rôle. It should be emphasized at the outset that not only are the conditioned response principles (*e.g.*, reinforcement, generalization) here employed well established and generally accepted, but their validity as principles of linguistic behavior has been determined by

[1] The present paper represents the theoretical background for research being conducted by the writers under a grant-in-aid from the American Association for the Advancement of Science.

[2] The writers wish to express their appreciation to the following persons who read and criticized a preliminary copy of the present paper: Professor C. H. Graham, Professor E. R. Guthrie, Professor C. L. Hull, Professor J. McV. Hunt, Professor W. S. Hunter, Dr. Margaret Keller, Dr. F. A. Mote, Professor Harold Schlosberg.

direct experimentation, as will be noted from the survey of related experiments below, although the implications of such experiments for psychological theory do not appear to have been fully recognized.

Language is here regarded as a form of conditioned behavior—both on the part of the speaker and on the part of the person spoken to. This objective psychological approach to language behavior differs considerably from the philological and related approaches in which the major interest is in language morphology, *i.e.*, language as things—isolated words or syllables. It also differs fundamentally from the logical discipline of semiotics (*cf.* Morris, **16**), which is concerned with the systematic elaboration of the meta-language in terms of which all sign situations may be classified and discussed. And it should be further emphasized that the study of language-as-conditioned behavior has no traffic with mentalistic theories which regard language as a means of 'expressing ideas.' For a comparison of these different approaches to language as well as for an objective psychological treatment of grammar, the reader is referred to Kantor (**8**).

I. Experimental Background and Operational Analysis

In the following paragraphs, we shall begin with a brief consideration of the phenomenon of generalization, followed by a discussion of the distinction between mediated and non-mediated (physical) generalization. Attention will then be directed to the problem of mediated verbal generalization,[3] after which the experiments in this field will be classified and the more relevant ones briefly summarized. The present section will conclude with a theoretical analysis of mediated generalization as a specific conditioned response phenomenon.

The phenomenon of generalization or irradiation of conditioned responses refers to the empirically determined fact that if a conditioned response is established to a given stimulus

[3] We recognize the possibility of linguistic mediated generalization gradients of a non-verbal nature, as in the case of sign and gesture languages. Mediated generalization in some form undoubtedly occurs in such instances, and it would be interesting to study the generalization gradients in these language systems.

(S_1), other stimuli (S_{2-n}) of the same dimension will elicit the response without reinforcement, the magnitude of the generalized response varying inversely with the distance of S_{2-n} from S_1 along the given dimension.[4] Such stimulus equivalence or gradients of generalization have been experimentally demonstrated in the case of various stimulus dimensions, such as the pitch or loudness of tones, as determined by psychophysical experiments, and the spatial distance of tactual stimuli applied to the body surface.

Several investigators have recently shown that generalization occurs along a *semantic gradient*, although the mechanisms involved need to be clarified and extended by theoretical implication. If we omit all other experiments in which conditioned responses have been formed to verbal stimuli [5] and consider only those directly concerned with semantic generalization, we may classify the investigations into three major categories.[6] The first of these comprises *experiments in which a conditioned reaction has been established to a stimulus object (denotatum) and generalization obtained to its name (sign)*. Positive results of this type have been reported by Kapustnik (9), Keller (10), Kotliarevsky (11, 12), Nevsky and Levin (17), Razran (19), Smolenskaya (27), Traugott (29), and Traugott and Fadeyeva (30). Conversely, Kapustnik (9) and Shastin (25), have described *experiments in which conditioned reactions have been established to a word (sign) and generalization obtained to its object (denotatum)*. A third

[4] We are not concerned here with the phenomenon of response equivalence, or response generalization.

[5] Noteworthy among such studies have been those on the conditioned acquisition of voluntary control of previously involuntary responses, as well as those on the effect of positive and negative attitudinal factors on conditioning. Although both of these areas of investigation are relevant to the present problem of semantic generalization, no attempt will be made to consider them here. Similarly, the experiments on indirect or mediated conditioning (Shipley, N. E. Miller, Lumsdaine) are arbitrarily omitted from the present discussion, owing to the fact that the mediated generalization is not semantic in character.

[6] In his description of methods for studying conditioned reflexes in man, Ivanov-Smolensky (7) includes a discussion of verbal conditioning, but neither the original monograph nor a complete abstract was available to the writers. References to two other volumes edited by Ivanov-Smolensky, and presumably including a summary of experiments on verbal conditioning and semantic generalization performed by students in his laboratory, are cited by Razran (21, p. 90, footnotes 1 and 2).

type of experiment has been concerned with the *establishment of a conditioned reaction to one word (sign) and generalization to other semantically and phonetically related words (signs)*. Experiments by Diven (3), Keller (10), Razran (20, 21, 22), Riess (23, 24), and Wylie (32) are concerned with this problem. Since they are most relevant to the present paper, these experiments may be briefly described.

Razran (20) measured the amount of saliva secreted while he thought of 'saliva' in English, Russian, German, French, Spanish, and Polish. As controls he used the Gaelic word for saliva, with which he was unfamiliar, a pair of nonsense syllables, and a period of 'blank consciousness.' A quantitative index of his familiarity with these languages was obtained through his speeds of reading and of association in each. The results indicated that the largest amount of saliva [7] was secreted for the Russian word, the language of his childhood and adolescence, in spite of the fact that he is now more fluent in English. The German, French, Spanish, Gaelic, and Polish words yielded decreasing amounts of saliva in this order, which was the order of his familiarity with the languages, except for a reversal between Gaelic and Polish. Thus the degree of generalization appeared to vary as a function of his knowledge of or frequency of use of the specific languages, although the results, obtained on a single subject, are tentative.

A quantitative study of semantic conditioning in three adult human subjects is elsewhere reported by Razran (21). An attempt was made to separate the semantic or meaning-content factor from the phonetic or visual-auditory form of the word by the use of synonyms and homophones. Four stimulus words (*style*, *urn*, *freeze*, *surf*) were flashed on a screen while the subjects were eating, the amount of salivation to each of the stimulus words and subsequently to each of the homophones (*stile*, *earn*, *frieze*, *serf*) and synonyms (*fashion*, *vase*, *chill*, *wave*) being tested.[8] The mean generalization was

[7] Measured by means of Razran's 'cotton technique,' which consists in ascertaining the increment in weight of an absorbent dental cotton roll inserted under the subject's tongue for a period of one minute.

[8] *Ibid.*

59 per cent to the synonyms and 37 per cent to the homophones, thus indicating that the verbal conditioning was largely semantic.

Riess (23) has repeated Razran's experiment, using the same stimulus and test words but employing the galvanic skin reaction (GSR) to a loud buzzer as a substitute for the salivary technique. The number of subjects varied from 4 to 9 in the case of the four stimulus words. Riess also found generalization to be greater to the synonyms than to the homophones, the mean percentage gain from conditioning being 94.5 for the homophones and 141.0 for the synonyms. In a recent unpublished study Riess (24) has included antonyms and has studied generalization at different age levels. Results appear to indicate that in young children generalization is greater to the homophones than to the synonyms, with the antonyms intermediate between the two, thus suggesting the relatively greater importance of phonetic as compared with semantic factors in the behavior of young children. With adults, however, the relationship is reversed, the order of increasing amount of generalization being: homophones, antonyms, synonyms.

Wylie (32) repeated the experiment performed by Razran and Riess, conditioning the GSR (elicited by shock) to certain words and testing generalization not only to homophones and synonyms of these words but also to other words (controls) bearing no relationship to the conditioned words. She found evidence of greater generalization to the homophones and to the synonyms than to the control words, but her results indicated greater generalization to the homophones than to the synonyms. This variation from the findings of Razran and Riess may, perhaps, result from the fact that her subjects said they expected shock on the words that sounded like the conditioned words but not on the synonyms of those words. Had her subjects been misled as to the nature of the experiment, this result might not have occurred. Wylie further studied semantic generalization by using nonsense syllables. Her subjects learned several pairs of syllables by the paired associates method, and the first members of two of these

pairs were then conditioned. Generalization was found to be much greater to the syllables associated with the conditioned syllables than to those syllables associated with non-conditioned syllables.

Another experiment by Razran (22) followed the same general method as that described three paragraphs above. For single-word conditioning, semantic factors were again found to be the most important determiners of the amount of salivary generalization in the adult subject, although 'phonetographic' relationships (sound-spelling, *e.g.*, *dark*, *mark*) were also very significant, especially at the beginning of the conditioning and after long continued pairings. Syntactic generalization was also studied by conditioning a salivary reaction to a 3-word sentence (consisting of subject, copula, and predicate) and then testing for generalization to other 3-word sentences in which one or more of the three words was reversed—giving rise to 7 verbal variations and to 4 contradictory and 3 concordant statements (*e.g.*, *Poverty is degrading*: *Wealth is degrading*, *Poverty is not degrading*, *Poverty is not uplifting*, *Wealth is uplifting*, etc.). Results indicated that the amount of generalization obtained depended upon the following factors (in decreasing order of strength of generalization): general agreement of statement, agreement of copula, predicate, subject; but reversals of copulas interfered more with transfer than general reversals of statements. Razran also reported that conditioned single words lost some of their generalization-strengths when combined in sentences, and that subjects' opinions regarding the truth of the statement influenced the conditioning as well as the generalization value of the sentence.

In a study designed to investigate the conditioning of anxiety reactions (GSR) to words, Diven (3) found generalization spreading 'by meaningful relation' to other words in the series. For example, from the reinforced word *barn*, generalization occurred to other words in a 'rural' category.

In a somewhat different experiment, still in its preliminary stages, Keller (10) has found that when the GSR is conditioned to the picture of an object, there is generalization

to the name of the object. She has also shown that a conditioned response established to one picture of an object (*e.g.*, a hat) will be generalized to the picture of another object in the same category (*e.g.*, another hat). In part, this last result might be attributed to physical similarity of the pictures of the objects, but there may also be a factor of conceptual identity in the different pictures which is in part responsible for the generalization.

Let us now turn to a consideration of mediated as compared with non-mediated generalization. It will be noted that *the dimension along which generalization occurs in the experiments described above is not a dimension characteristic of the physical attributes of the stimuli.* The classical formulation of generalization principles presupposes naive organisms (*cf.* Hull, **5**), whereas *mediated* (*e.g.*, *semantic*) *generalization depends upon previously conditioned* (*usually language*) *behavior.* Razran (**19**, pp. 70–71) implies this distinction in his 'polymodal concept' of human conditioning, according to which the mean amount of conditioning is held to be a function of (1) a *physiological factor* dependent upon the 'biological potency' of the stimulus and its correlated 'physiological reaction-change' and (2) a *psychological factor* resulting from the associational history and patterning of the stimulus. A similar distinction has been made by Hull (**6**) in a discussion of the problem of stimulus equivalence in behavior theory.[9] Hull suggests that the three mechanisms mediating stimulus equivalence are: (1) the *partial physical identity* of the stimulus compounds, (2) *primary or physiological generalization* (irradiation), and (3) *secondary or indirect generalization* through the arousal of a reaction previously conditioned to the same stimulus continuum. Hilgard and Marquis (**4**, p. 201, italics ours) summarize the problem of generalization and the definition of stimulus similarity as follows:

> There are a number of types of equivalence between stimuli (and the responses which they evoke) which cause the stimuli to be reacted to *as* similar, so that conditioned responses formed to one

[9] Many of these distinctions bear a resemblance to Kantor's interbehavioristic 'stimulus function.'

of the stimuli will be generalized to the other. Among these may be mentioned: *partial identity* (whereby stimulus A–B is similar to A–C); *sensory similarity* (so that two colors are more alike than a color and a tone); *formal similarity* (for example, two patterns of ascending pitch); *affective similarity* (equivalence mediated by emotionally flavored processes, as in the "dark brown taste" or "blue Monday"); and *mediated similarity* (as in the equivalence of words of different sounds, but of common meanings). Of these relationships, only the first (partial identity) may be conceived of as physical similarity. The second (sensory similarity) may be mediated by relatively primitive physiological processes, as represented in the classical experiments on generalization within a sensory modality. The others all depend upon relatively complex processes, many of which require previous learning in order that the stimuli involved may be responded to as similar.

It is with this final type of 'mediated' generalization that we are here concerned. It is clear that semantic generalization would not take place in the case of a subject who did not know the meaning of the words or who saw no conceptual identity or similarity in the pictures of the hats in Keller's experiment. The mediated or semantic generalization in such experiments thus presupposes and depends upon the pre-experimental formation of conditioned responses or associations, *i.e., the gradient of generalization is a gradient along a dimension of conditioned stimulus functions.* The stimuli need be similar only in so far as they have been previously conditioned to the same (or similar) response.

A simplified schema of such mediated or semantic generalization is suggested in Fig. 1. At some pre-experimental time, the subject has become conditioned, either by direct reinforcement or by higher order conditioning, to make R_x to CS_1, CS_2, CS_3 . . . CS_n. The subject is next experimentally conditioned by reinforcing CS_1 with UCS_y, setting up a conditioned response, R_y. Generalization is now found to CS_2 . . . CS_n, the magnitude of the generalized response presumably depending upon the relative strengths of the pre-experimental conditioning of CS_1 . . . CS_n to R_x.

It should be emphasized that the schema in Fig. 1 is an operational account of what descriptively occurs in mediated generalization. Hypotheses regarding further, non-observ-

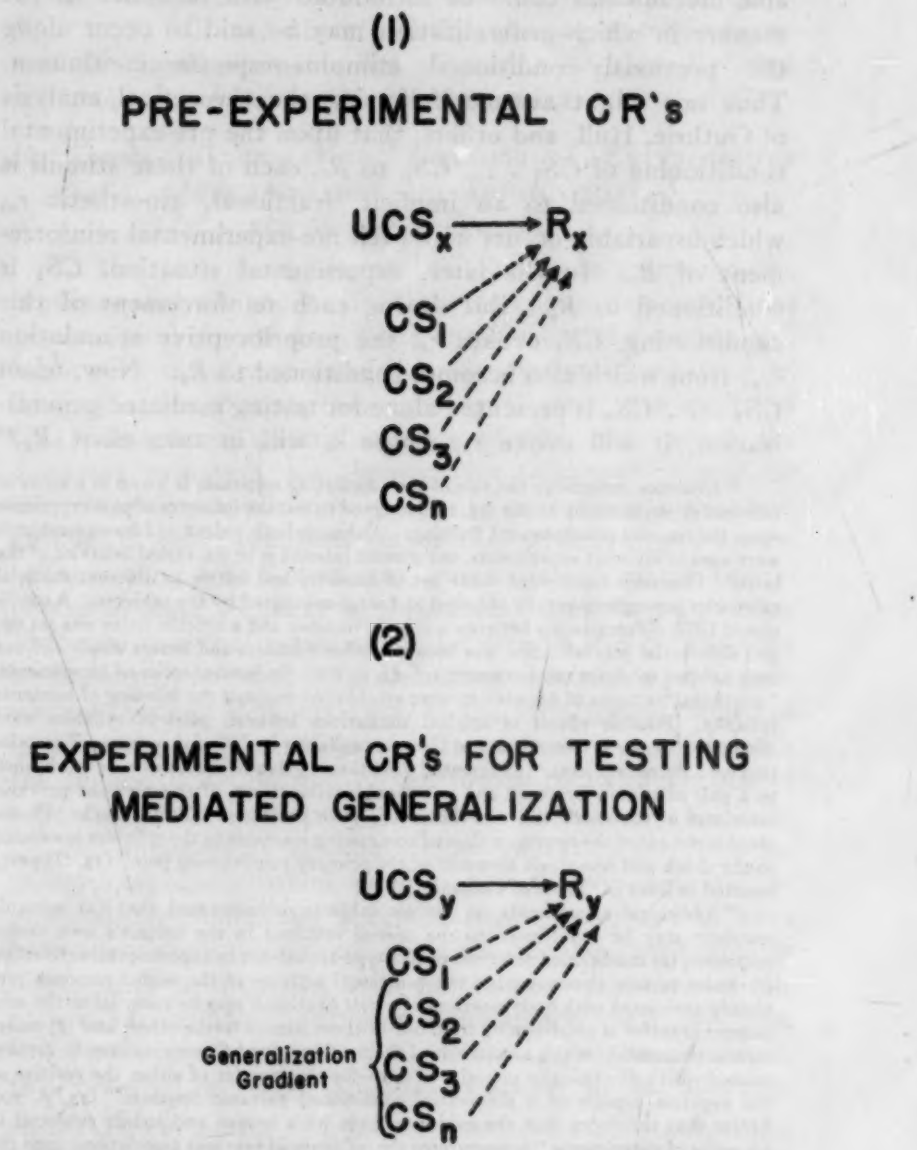

FIG. 1. Simplified descriptive schema for the development of mediated generalization.

able mechanisms could be introduced with reference to the manner in which generalization may be said to occur along the previously-conditioned stimulus-response continuum. Thus one might assume, following the theoretical analysis of Guthrie, Hull, and others, that upon the pre-experimental conditioning of $CS_1 \ldots CS_n$ to R_x, each of these stimuli is also conditioned to an implicit, fractional, kinesthetic r_x, which invariably occurs with each pre-experimental reinforcement of R_x. In the later, experimental situation, CS_1 is conditioned to R_y. But during each reinforcement of this conditioning, CS_1 evokes r_x, the proprioceptive stimulation (s_x) from which also becomes conditioned to R_y. Now, when $CS_2 \ldots CS_n$ is presented alone for testing mediated generalization, it will evoke r_x, whose s_x will in turn elicit R_y.[10]

[10] Evidence concerning the rôle of such mediating responses is found in a series of different experiments by Miller (13, 14), designed to test the influence of past experience upon the transfer of subsequent training. Although both rodent and human subjects were used in different experiments, our present interest is in the verbal behavior of the latter. One such experiment made use of numbers and letters as different material categories pre-experimentally acquired and inter-associated by the subjects. A conditioned GSR differentiating between a specific number and a specific letter was set up, and differential generalization was found to other numbers and letters which had not been subject to direct reinforcement (*cf.* 13, p. 70). In further series of experiments, "controlled patterns of association were established through the learning of nonsense syllables. Possible effects of original similarities between pairs of syllables were eliminated by the systematic use of the same syllables in different patterns of association for different subjects. Differential conditioned galvanic reactions were established to a pair of stimuli by shock and non-shock combinations, after which the previous associates of the shock and non-shock pair were presented without shock. Photographic records of the responses showed contrasting reactions to the syllables associated to the shock and non-shock elements of the primary conditioning pair" (13, 'Digest,' inserted in front of 'Table of Contents').

"Additional experiments on human subjects demonstrated that (*a*) galvanic reactions may be conditioned to the stimuli involved in the subject's own verbal responses; (*b*) conditioned reactions of this type are subject to experimental extinction; (*c*) under certain circumstances, the functional activity of the verbal response previously associated with both members of a pair of stimuli may be essential to the subsequent transfer of conditioning from one of these stimuli to the other; and (*d*) under certain circumstances this crucial rôle of the mediating verbal response may be demonstrated—with dynamically opposite effects—for the transfer of either the positive or the negative aspects of a differential conditioned galvanic response" (13, p. 70). Miller thus concludes that the evidence (from both human and rodent subjects) in his series of experiments "demonstrates the influence of previous associations upon the generalization of subsequent conditioning and indicates that mediating responses can play an important rôle in this transfer" (13, 'Digest').

Miller and Dollard (15, p. 77) report an unpublished dissertation by Birge (1) in which behavioral generalization in children was found to be mediated by verbal responses. Birge found that "if young children are taught to call two very different

Thus, from a purely molar or descriptive point of view, $S_2 \ldots S_n$ will lead directly to R_y. A diagrammatic representation and more detailed discussion of these inferred mechanisms are presented by Hull (**6**, pp. 27–28).

II. Formal Illustration, Methods of Experimental Demonstration, and Implications for Behavior Theory

In this section an attempt will be made to extend the previous analysis and to illustrate its application to various psychological situations. More specifically, the section will begin with the discussion of a formal system of notation for designating mediated (*i.e.*, synonym) and non-mediated (*i.e.*, homophone) generalization gradients, after which the discussed synonym and homophone relationships will be illustrated by application to an hypothetical subject. A number of other gradients (other than synonym and homophone) along which mediated generalization might be expected to occur will next be suggested. A minimum number of formal concepts essential to the present theory will then be presented, followed by a discussion of methods for experimental check or demonstration of the expected relationships in the case of a given subject. The paper will conclude with the suggested application of the theory to various fields of psychological investigation.

The implications of semantic generalization for behavior theory, especially when combined with non-semantic or physical (*e.g.*, phonetic) generalization, appear not to have been adequately recognized. The relationships explicated in Fig. 1 may be regarded as a generalized statement of the nature of mediated stimulus equivalence. We are here interested in one form of mediated generalization, *viz.*, that involved in verbal language behavior; we are also con-

stimulus objects by the same name, other responses (such as reaching for the objects) are more likely to generalize from one to the other than when the two objects have been given different names." Birge likewise noted that "such generalization is much more likely to occur when the children say the name aloud, so that it is certain that the cue-producing response is actually present, than when they do not say the name aloud" (**15**, p. 77). Miller and Dollard (**15**, pp. 74–78) also discuss the rôle of the mechanism of mediated generalization in various forms of adult social behavior.

cerned with non-mediated generalization insofar as it plays a part in these problems.

In Fig. 2 we have suggested certain formal relationships

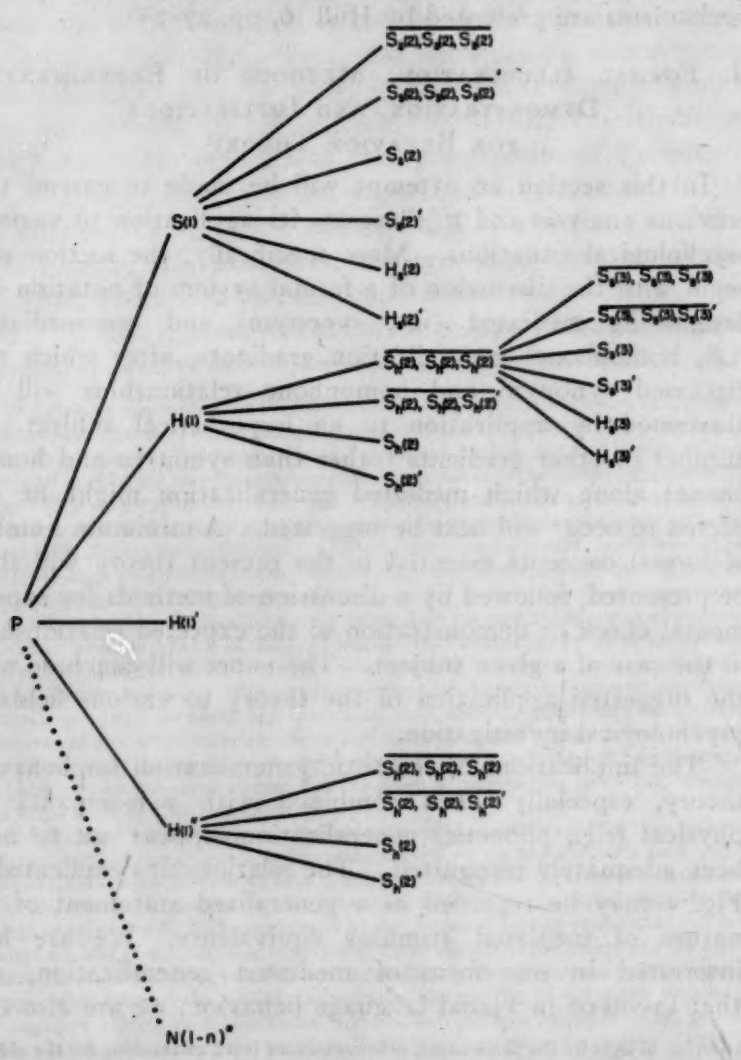

FIG. 2. Formal illustration of certain phonetic and semantic (mediated) generalization gradients in hypothetical subject.

* For experimental purposes it is necessary to have a neutral (control) word, *N*, at every stage in the diagram; a set of such neutral words bearing the relationship of synonymity or homophonity to each other could also be utilized as additional controls.

which might be expected to obtain in the case of an organism whose language behavior is highly developed. Mediated generalization is here depicted in terms of synonymity of words and non-mediated generalization in terms of homophonymity of words. The following set of definitions will explain the system of notation used in this figure:

P refers to the original, reinforced stimulus (presentation) word.
S refers to a synonym of *P*.
H refers to a homophone of *P*.
N refers to a neutral word.
Numbers in parentheses designate the number of semantic or phonetic transformations removed from *P*.
S_s refers to a synonym of the preceding synonym.
S_h refers to a synonym of the preceding homophone.
H_s refers to a homophone of the preceding synonym.
(No notation is required for a homophone of a preceding homophone. *Cf.* below.)
A prime (′), or primes (″ or ‴), is used to differentiate various words or various sets of words which are synonyms or homophones of the preceding word but not synonyms or homophones of each other.
A long bar (——) is used to cover a set of words (synonyms) which are also synonyms of each other. It is extremely unlikely that there will be a set of homophones which are synonyms of each other.
It is evident that if two or more words are homophones of a preceding word they will be homophones of each other. It is extremely unlikely that there will be a set of synonyms which are homophones of each other.

It will be noted that Fig. 2 is a formal representation of some of the possible relationships arising from certain phonetic and semantic (mediated) generalization gradients. A given word, *P*, is presented and reinforced. According to the data of Diven (3), Razran (21, 22), Riess (23, 24), and Wylie (32), synonomous words, when presented, will likewise elicit a response, though less in degree. Now *P* may have only one synonym or it may also have several synonyms; in the latter case these several synonyms may not bear to *P* the same degree of semantic relationship, *i.e.*, one word (synonym) may be more completely interchangeable with *P* than the others although all must bear some degree of interchangeability

with *P*. (In this connection it should be noted that language authorities seldom assert that two words have precisely the same meaning. It seems likely, therefore, that the foregoing statement is sound.[11]) Therefore, we may have a series of words, varying in semantic similarity to *P*, which would be likely to elicit different degrees of response as a function of the degree of their similarity to *P*.[12]

Furthermore, *P* may have more than one meaning; *e.g.*, the word *vain* has both the meaning of *conceited* and of *fruitless*. Hence, it is possible to conceive the reinforcement of *P* as causing generalization effects along more than one semantic series.

Another possibility for generalization from the reinforced word (*P*) is provided by the homophone relationship.[13] For example, if *vain* is the reinforced word, the effects of this reinforcement may spread to *vein* and to *vane*. We cannot be certain that these words would elicit different amounts of response to form a definite gradient, but such factors as relative familiarity of the homophones (*cf.* Razran, **20**) and degree of similarity in spelling might cause a definite gradient to appear.[14] At any rate, we are probably justified in assuming that generalization would occur to homophones, whatever the type of curve.

Deriving from the foregoing generalization gradients are several other possibilities:

[11] It is necessary to remember that the relation of synonymity in the language behavior of persons is dependent upon previous associations (conditioning). Hence, we cannot expect generalization gradients to picture the dictionary semantic relationships but should expect them to vary with the particular individual in the light of his language history. *Cf.*, *e.g.*, Mosier, C. I., A psychometric study of meaning. *Psychol. Bull.*, 1939, 36, 607–608, and *J. soc. Psychol.*, 1941, 13, 123–140.

[12] *Ibid.*

[13] There are many possibilities here. In the text we are chiefly concerned with words, conventionally designated as homonyms or homophones, which are alike in sound but not in spelling and meaning. Homographs could also be considered, both when the words have the same sound and spelling but different meanings (*e.g.*, *fair, beautiful* and *fair, market*) and when the words are spelled alike but differ in both sound and meaning (*e.g.*, *bow, knot* and *bow, forward part of a vessel*). Likewise, we have said nothing concerning the place of the antonym in this picture. Generalization along each of these (and other) continua could readily be investigated.

[14] It should also be noted that partial identity in sound or spelling might be adequate for some generalization to appear. For example, if *lamp* be the reinforced word, generalization might appear to such words as *tramp, cramp, camp*, etc. *Cf.* Razran (22).

1. From each of the synonyms of the original word there will be possible homophone gradients. Also, in the event that any of these synonyms has more than one distinct meaning, there will be in addition gradients formed by the synonyms arising from the different meaning, as well as homophone gradients arising from each of the members of the new synonym series.

2. Each of the homophones arising from the synonyms described above in 1 also may have synonyms, causing additional synonym gradients to appear; these synonyms may have homophones, more than one meaning, etc. The number of possible gradients is thus extended to a very large number.

3. Each of the members of the series of words homophonous with respect to *P* may have synonyms,—several sets of synonyms in the cases of words having more than one meaning. Each of these synonyms may have homophones, and so on.

With these statements and comments as an introduction, we may now examine Fig. 3, which illustrates the application of the relationships discussed in Fig. 2 to a concrete but hypothetical example. In discussing this diagram, certain important considerations must be kept in mind. The semantic and phonetic gradients portrayed are *limited* to synonym and homophone relationships, and for purposes of clarity, only *certain* of these relationships have been suggested. Moreover, the relationships or dimensions shown are purely *formal*, whereas those involved in the case of actual subjects would be distinctly idiosyncratic—varying as the result of the individual subject's particular linguistic biography. Fig. 3 thus pictures certain formal relationships, or generalization gradients, which, it is assumed, may be *simultaneously* activated (strengthened) upon the reinforcement of *P*. In the following descriptive text, the italicized words are the words found in Fig. 3, and the symbols indicate where they would appear in Fig. 2.[15]

[15] To a certain extent the relationships suggested in Fig. 3 have also been suggested by Woodworth (31, pp. 36–37), although less systematically, in his discussion of 'mediate' or 'indirect recall.' *Cf.* a later section of this paper.

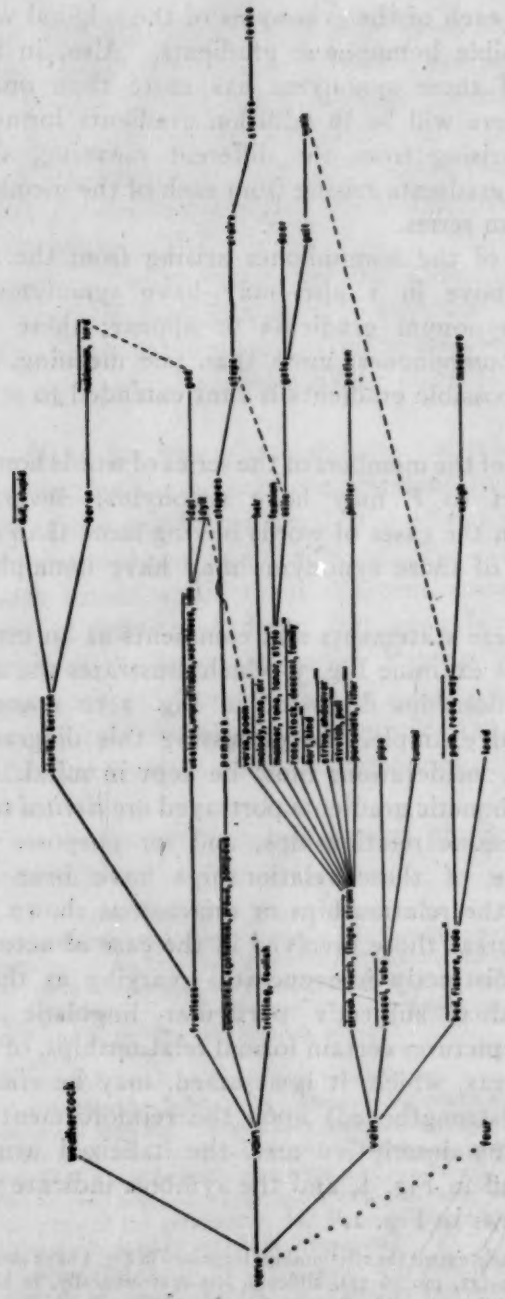

FIG. 3. Formal representation of certain possible phonetic and semantic (mediated) generalization gradients.

The presentation word *vane* (*P*) is taken as the original, reinforced stimulus from which generalization would be expected to occur along the synonym gradient leading to *weathercock* ($S_{(1)}$) and along homophone gradients leading to *vain* ($H_{(1)}$) and to *vein* ($H_{(1)}'$). The word *four* ($N_{(1)}$) is inserted as a neutral or control word. From *vain*, three different synonym gradients are shown, the first leading to *fruitless* ($S_h(2)$), another to *conceited*, *egotistical*, *proud* or *pompous* $\overline{(S_h(2), S_h(2), S_h(2), S_h(2))}$, and a third to *trifling* or *foolish* $\overline{(S_h(2), S_h(2)')}$. *Pompous* bears a quasi-homophonic relationship to *pampas* ($H_s(3)$), from which alternate synonym and homophone gradients lead consecutively to *steppes* ($S_h(4)$), *steps* ($H_s(5)$), *stairs* ($S_h(6)$), *stares* ($H_s(7)$), and *looks* ($S_h(8)$). Two synonym gradients are depicted from *fruitless*. The one to *sterile* or *barren* $\overline{(S_s(3), S_s(3))}$, leads from *barren* to its synonym, *dull* or *stupid* $\overline{(S_s(4), S_s(4))}$, and to its homophone, *baron* ($H_s(4)$), whose synonym is *nobleman* or *lord* $\overline{(S_h(5), S_h(5))}$. The other synonym gradient from *fruitless* leads to *useless*, *unprofitable*, *worthless* or *idle* $\overline{(S_s(3), S_s(3), S_s(3), S_s(3)')}$, the homophone gradients from *idle* leading to *idol* ($H_s(4)$) and to *idyll* ($H_s(4)'$); whereas the synonym gradient from *idol* leads to *god* ($S_h(5)$). It is of some theoretical significance to note, as shown by the broken line in Fig. 3, that *god* is a synonym of *lord* ($H_s(5)$), whose associative strength is derived through *baron* ($H_s(4)$) and *barren* ($S_s(3)$) (*cf.* above); thus a circularity is achieved in the suggested semantic and phonetic gradients, and the words *god* and *lord* would be expected to show intensified magnitudes owing to algebraic summation of their reaction potentials (*cf.* below).

From *vein* ($H_{(1)}'$), another homophone of the original presentation word *vane* (*P*), three different synonym gradients are represented in Fig. 3. One leads to *bed*, *fissure* or *lode* $\overline{(S_h(2), S_h(2), S_h(2)'')}$, with *lode* furnishing the homophone, *load* ($H_s(3)$) and the synonyms, *path*, *road* or *way* $\overline{(S_s(3), S_s(3), S_s(3))}$, the last named leading to its homophone, *weigh* ($H_s(4)$) and thence to the latter's synonym, *measure* ($S_h(5)$). A second gradient from *vein* leads to *vessel* or *tube* $\overline{(S_h(2), S_h(2)')}$, the latter having the two additional synonym

gradients terminating in *pipe* ($S_s(3)$) and *tunnel* ($S_s(3)'$), respectively. The third synonym gradient represented from *vein* leads to *streak*, *touch*, *tinge* or *strain* ($\overline{S_A(2), S_A(2), S_A(2), S_A(2)}$), from the last word of which the following eight synonym gradients (all $S_s(3)$) are derived: *verse* or *poem; melody*, *tune* or *air*—leading to the last's homophone *heir* ($H_s(4)$); *manner*, *tone*, *tenor* or *style*—leading to the last's synonym, *fashion* ($S_s(4)$), and homophone *stile* ($H_s(4)$), with its own synonym, *gate* ($S_A(5)$) and its homophone, *gait* ($H_s(6)$) and the latter's synonym, *walk* ($S_A(7)$); *race*, *stock*, *descent* or *family; sort* or *kind; strive* or *endeavor; stretch* or *pull;* and *percolate* or *filter*—leading to the latter's homophone, *philter* ($H_s(4)$), and its own synonym, *potion* ($S_A(5)$). Attention is also called to the fact that *poem* ($S_s(3)$) is a possible synonym of *idyll* ($H_s(4)'$), as indicated by the broken line in Fig. 3, which fact illustrates another case of circular or double reinforcement along different semantic and phonetic generalization gradients. Similarly, *stile* ($H_s(4)$) is a synonym of *steps* ($H_s(5)$), and *way* ($S_s(3)$) is a synonym of *walk* ($S_A(7)$). These examples of circular generalization gradients differ from the *god-lord* relationship discussed in the preceding paragraph in that in the present instances the reaction potentials of the two words (*poem* and *idyll; stile* and *steps; way* and *walk*) are of different theoretical magnitudes, since they differ in the number of semantic and phonetic transformations removed from the original, reinforced presentation word, *vane*.

As was previously pointed out, the above gradients suggested in Figs. 2 and 3 have been limited to *certain of the formal synonym and homophone relationships* assumed to obtain in the case of the original, experimentally reinforced presentation word. In the actual experimental situation, many additional gradients will obviously operate concurrently. The following list of such formal gradients,[16] or formal dimensions along which generalization may be expected to occur, may serve to indicate this semantic and

[16] This list follows, with modification, the classification of responses in the free association experiment, as suggested by Jung and adapted by Wells, F. L., A preliminary note on the categories of association reactions, PSYCHOL. REV., 1911, 18, 229–233.

phonetic multiplicity: egocentric (*success—I must*); egocentric predicate (*lonesome—never*); evaluation (*rose—beautiful*); matter-of-fact predicate or substance-attribute (*spinach—green*); attribute-substance (*green—spinach*); subject-relation, including agent-action (*dog—bite*) and action-agent (*bite—dog*); object-relation, including verb-object (*shoot—deer*) and object-verb (*deer—shoot*); causality, including cause-effect (*joke—laughter*) and effect-cause (*laughter—joke*); coordination (*cow—horse*); subordination or genus-species (*fruit—apple*); supraordination or species-genus (*table—furniture*); contrast, opposites, or antonym (*black—white*); coexistence or contiguity (*Sunday—church*); identity or synonym (*blossom—flower*); diminutives (*man—boy*), and their converse (*boy—man*); phrase completing (*forward—march*); word completing or compounding (*black—board*); assonance, partial identity, or rhyme (*pack—tack*); syntactic change (*deep—depth*); homonym, including homophone (*vane—vain*) and homograph *a* (*fair, beautiful—fair, market*) as well as homograph *b* (*bow, weapon—bow, forward part of vessel*). In addition, generalization may be expected to occur along dimensions relating to other formal aspects of the words,[17] such as number of syllables, presence of prefix or suffix, presence and particular location of particular letters (*e.g.*, initial letter, as in the case of alliteration), as well as words associated with national, political, economic, 'racial,' social, occupational, fraternal, recreational, or purely 'personal' matters. In the case of the actual subject, of course, the existing mediated verbal generalization gradients will be idiosyncratic—the result of the individual's particular past conditioning. The above, formal

[17] Skinner (26, p. 96) refers to certain of these mediated generalization gradients, although from a different point of view and in a less systematic manner. He writes: "Perseveration is a figurative way of saying that when a member of a group is elicited, all members of the group are temporarily strengthened relative to their normal latent strengths. Thus, if I say *hire*, all my responses which resemble that response are temporarily strengthened—particularly the response *hire* itself, which is likely to be elicited again sooner than its resting strength would dictate, and *higher*, which leads me to pun. Also strengthened are all words beginning with *h* (hence alliteration) and all ending in *ire* (hence rhyme). As to perseveration of theme, my response *hire* strengthens all responses associated with its referent, and I am likely to 'go on' to speak of labor, wages, and so on."

gradients are thus hypothetical abstractions illustrative of some of the possible logical and philological relationships.

The foregoing discussion has been designed to suggest some of the possible interpretations of verbal behavior in the light of a systematic application of the concept of generalization to at least a part of this field. It is, of course, necessary to check, experimentally, the relationships postulated above; we shall deal with this matter in a later section of this paper. It first seems necessary, however, to outline a minimum number of formal concepts which might somewhat simplify the application of the previous analysis to areas of behavior in which language plays a part. At this stage in the development of our analysis, we have not thought it essential to set down a number of definitions and rigorously worded postulates, although we recognize both the desirability and the necessity for doing so ultimately. Therefore, we are here introducing only the most vital concepts which we regard as important to the further explication of the present theoretical and experimental analysis.

1. *Reaction potential.*—When an organism is introduced into a given stimulating situation, there may, originally, be no especial response apparent. Subsequent to the reinforcement of some *S-R* sequence, however, the organism, in the presence of this situation, will respond in a predictable way. Thus we may say that the organism has a *reaction potential.*[18]

2. *Strength of reaction potential.*—If two reactions are possible in a given situation and one is made instead of the other, the former may be said to have possessed a greater strength of reaction potential than the latter. Within the framework of the present schema, the reaction made by the organism at any given time will depend upon the relative strengths of the existing reaction potentials, the strongest reaction potential evoking its response if its strength is above the reaction threshold. If two reaction potentials of equal or approximately equal strength are present, there will be

[18] This term is similar in many respects to such terms as response tendency, reaction tendency, response potential, behavior tendency, behavior potential, excitatory potential, etc., used by other writers.

no response (inhibition, blocking) until spontaneous recovery or some other process causes one reaction potential to become stronger than the other.[19]

The strength of a given reaction potential at any moment may be assumed to depend upon the number of experimental reinforcements of *P*, the number of semantic and phonetic transformations it (the given word) is removed from *P*,[20] and the number of pre-experimental reinforcements of the given word (*cf.* Fig. 2). It is also possible that to the reaction potential of a given word would be added the pre-experimental reaction potential of *P* and that of each of the words preceding it along the continuum from *P*. Whether this is the case is a question for empirical determination.

Further, if a given reaction potential be derived from reaction potentials present as a result of the simultaneous activation of two or more gradients arising directly or indirectly from the reinforcement of *P*, as in the case of the god-lord, idyll-poem, gate-steps, and way-walk examples cited above, the strength of the reaction potential of this response will be intensified, since the positive reaction potentials from the different gradients may summate.[21]

3. *Reaction threshold.*—It may be assumed that for a response to occur, the strength of its reaction potential must reach a certain magnitude (and that positive potentials must exceed negative ones by this amount). This magnitude may be designated as the *reaction threshold* and for purposes of convenience may be regarded as equal for all verbal responses.

[19] For completeness, it would be necessary to assume a liminal difference between the strengths of two reaction potentials before one could evoke its reaction in the presence of the other.

[20] We may assume, for the sake of simplicity, a constant decrement in the generalization of reaction potential from *P* for each of the transformations a given word is removed from *P*. We recognize that this assumption may be too simple and that the amount of generalization of reaction potential may be a function, for example, of the pre-experimental reaction potential of the given word. Similarly, we have assumed that all reaction potentials at a given number of transformations from *P* will be incremented by a constant amount upon the reinforcement of *P*. This, also, may be too simple an assumption.

[21] We recognize that inhibitory gradients arising from the negative reinforcement (punishment) of *P* are possible. It may be assumed that the negative reaction potentials of members of such series will sum algebraically with such positive reaction potentials as may exist.

The application of the principle of generalization to language behavior is based upon the experimental evidence and logical inferences summarized above. The evidence thus far available has been secured by studying the subject's reactions without any especial effort to control or to secure information about his previous language history. In other words, the investigations have proceeded without a systematic attempt to discover the relative strengths of different reaction potentials at the moment of experimentation. In some ways this is an unsatisfactory situation. For this reason, as well as to indicate the manner in which we might go about determining whether Fig. 2 (or its special case, Fig. 3) is an adequate representation of the way in which generalization operates in the case of language behavior, we wish now to mention briefly four possible procedures for giving the experimenter some degree of knowledge concerning or control over the relative strengths of the verbal reaction potentials in the subject with whom he expects to work.

(1) The first method involves the pre-experimental reinforcement of certain verbal reactions, such that when the experimental session itself begins, the experimenter will have reason to expect that certain reaction potentials will be stronger than others.

(2) The second method involves the pre-experimental determination of the present strength of reaction potentials either statistically through Kent-Rosanoff frequencies, for example, or by actual measurements of word association tendencies in the subjects in question.[22]

(3) The third method, similar in some respects to the second, involves securing information concerning the subject's language biography, as was the case in Razran's experiment (20) cited above, in which the experimenter was able to assess the relative familiarity of the subject with different languages.

[22] The verbal summator, described by Skinner (26), could likewise be employed for this purpose. This method consists in the repeated presentation of skeletal speech samples which, through summation and 'imitation,' elicit the strongest latent speech responses of the subject.

(4) The fourth method involves setting up an artificial situation in which symbols (*e.g.*, nonsense syllables) having small (minimal) amounts of reaction potential could be used and differentially strengthened in a manner similar to that employed in Wylie's experiment (**32**, *vide supra*).

By employing such methods as these along with an involuntary response, such as the GSR, one should be able to determine fairly accurately whether the picture given in our theoretical diagrams resembles that present in an actual subject, or whether there are certain major inadequacies in our analysis.

On the basis of the foregoing analysis, it seems possible to integrate certain diverse phenomena ordinarily regarded in psychological writings as more or less unrelated. We have chosen a few such examples for brief comment. Although these examples have been selected from various traditional 'fields,' it is to be noted that the distinctions between these fields are somewhat arbitrary (and from our standpoint unsatisfactory).

1. *Perception.*—Perception is usually defined as the response to the 'meaningful character' of a stimulus in terms of past experience or in terms of the 'structural properties' of the field. From the point of view of the present writers, a perceptual response is a conditioned response; the response may have originated through direct or higher order conditioning or may have resulted from generalization. Undoubtedly much of this generalization arises from physical similarity of stimuli, although it may likewise occur frequently through semantic and related gradients (*cf.* above). Thus, for example, in certain experiments on the perception of tachistoscopically presented forms, the subjects' reports evidence modification of the original figure in the direction of a familiar (often verbalized) object. Perhaps many instances of the '*déjà vue*' phenomenon as well as other cases of recognition may also be accounted for through the processes of mediated generalization, although the factor of partial physical identity of the situations must also be recognized as important.

2. *'Set' or Einstellung.*—When a given performance is modified by events which precede it in time, this modification is often attributed to the 'set' of the subject, and the set is said to have resulted from the prior events. Many instances of 'set' are alleged to involve muscular adjustments, but other instances, particularly those involving verbal processes, are left unexplained (unless naming may be said to constitute an explanation!).[33]

It seems possible that the mechanism of mediated generalization may be operative in some of these cases. The writers are currently attempting to test this possibility by determining the number of words from a given list which can be reported by subjects after a single exposure; sometimes the test list is preceded by experience with synonyms of its members, whereas the control condition involves pre-test experience with unrelated words. A 'set' is thus built up in the one case, or, as we have phrased it, the activation of semantic gradients through the presentation of the synonyms should result in a superior performance on the related list. A practical example of this sort is found in the memory span test, in which the results might be influenced by the pre-test verbal behavior of both subject and examiner. Thus, the memory span for disconnected words might be greatly modified if some of their synonyms chanced to be employed before the actual administration of the test or if different words in the series fell on the same or mutually reinforcing gradients.

3. *Learning.*—Many problems in the psychology of learning may likewise be clarified by this analysis, at least in the case of problems in which meaningful words—discrete or connected—are employed. One of us (2) has previously suggested that gradients of the sort described above may operate to cause at least some of the errors made in prose learning.[34]

[33] The importance of 'set,' attitudinal factors, and the like has been demonstrated in a number of conditioning experiments; such factors undoubtedly operate though instructions, 'sophistication,' and implicit self-administered stimulation, both before and during the course of many other kinds of experiments.

[34] *Cf.* also Thorndike's (38) analysis of the possible rôle of phonetic and semantic factors in producing errors during the learning of English equivalents of French words.

4. *Memory* (*Indirect recall*).—One example will perhaps suffice to illustrate the application of the present theory of mediated verbal generalization to memory behavior. In his discussion of memory, Woodworth (31, pp. 36–37) has described a process which he has called indirect or mediated recall. Thus in recalling the second of two paired associates, the subject may report the use of intermediary steps such as meaningful relation. For instance, in the case of the pair, *above-conceal*, the word *ceiling* may intervene, being similar in meaning to *above* and similar in sound to *conceal.* These relationships would seem explicable in terms of semantic gradients in the first step and in terms of quasi-phonetic gradients in the second. Other memory phenomena, such as perseveration, might also be clarified by this type of analysis.

5. *Intelligence.*—While a detailed analysis has not been attempted, it seems possible that the gradients here described may be important in differentiating various degrees of 'intelligence,' as measured by the usual verbal intelligence tests. Thus, the level of performance on such tests may well be influenced by the number and type of semantic transformations which a subject, by virtue of his previous learning, is able to accomplish.

6. *Reasoning.*—Success in reasoning situations is often regarded as a function of the number of modes of attack with which a subject may approach the problem, or as related to the variety of approaches he exhibits in discovering a solution. It may perhaps be suggested that in the verbal sphere, at least, the number and type of semantic transformations which the subject is able to accomplish is an important determinant of the reasoning process.

7. *Free association.*—Free association, whether in laboratory or everyday life situation, offers an obvious illustration of mediated verbal generalization. Beginning with a stimulus word or other stimulus material, the course of the responses may be regarded as determined by the language relationships peculiar to a given subject (according to the dimensions above indicated) and by the different strengths of the existing reaction potentials. Other language relationships not strictly

determined by phonetic and formal or institutionalized semantic factors (*i.e.*, those associations peculiar to any one individual's reaction history) likewise play a part.[25] A similar statement may be made with respect to reverie and other imaginative behavior. Non-verbal stimulation, such as pictures, scenes, and the like, may well initiate a series of vocal (or sub-vocal) reactions in these instances.

8. *Emotional behavior.*—A suggestion may also be made as to the possible importance of the mechanisms here described in the case of emotional behavior. If a traumatic experience (to take a relatively simple example) be accompanied by a chance event, *e.g.*, the lighting of a cigarette, a whole series of words may subsequently cause anxiety or fear (neurotic) behavior. Thus, generalization could occur from *match* to such words as *wedding*, *marriage*, *wedlock*, *nuptial*; from *light*, generalization might take place to *lamp*, and, through assonance or partial identity, to *tramp* and finally to *hobo*. In the last instance the individual would probably be unable to report the reasons for his fear of hoboes ('unconscious anxiety,' *sic*) and a lengthy investigation at best might be necessary to get at the basis for the fear.[26] From this point of view, the free association technique may be regarded as a diagnostic method for studying the patient's particular semantic gradients in order to determine the biographical events leading to his aberrant conditioning. It is interesting to note that other so-called psychoanalytic mechanisms and techniques might perhaps be treated in a similar manner.

[25] In this connection it is important to note that even if one assumes a fixed number of semantic and phonetic dimensions and a purely formal language history, the individual may give consecutive verbal responses which bear no relationship *to each other* along any of these dimensions. Such consecutive apparently unrelated responses frequently occur in continuous free association experiments. From our discussion of the theoretical basis of semantic and phonetic generalization, it will be apparent that such superficially unrelated responses (*e.g. load* and *baron* in Fig. 3) might lawfully occur provided their reaction potentials were of sufficient strength at consecutive intervals of time.

[26] A fascinating field for investigation is here suggested in connections with Mowrer's discussion of the motivational value of anxiety in learning. This problem, however, is beyond the scope of the present paper. *Cf.* Mowrer, O. H. A stimulus-response analysis of anxiety and its rôle as a reinforcing agent. PSYCHOL. REV. 1939, 46, 553–565.

We could multiply indefinitely these instances of the type of psychological inquiry to which the application of the theory here advanced would seem to promise fruitfulness. And it should be noted that most of these questions are capable of direct experimental test, at least in principle. Indeed, many relevant data are already available in the literature, and need only be summarized in the light of the present systematization in order to bring considerable clarity and unity into otherwise disparate and unsystematized areas of investigation.

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'HYPOTHESES' IN RATS: AN EXPERIMENTAL CRITIQUE. III. SUMMARY EVALUATION OF THE HYPOTHESES CONCEPT

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I. Introduction

In the 'hypotheses' concept of Krechevsky is to be found what is perhaps the most ambitious application of purposivistic theory thus far attempted. Based upon the results of studies with discrimination situations the concept was formulated in the following terms:

> When we say that an individual is working on an 'hypothesis,' we at once imply that he is behaving in a purposive, 'if-then' manner. An 'hypothesis' must be put to experimental test, an hypothesis is something that must be *verified* before it is persisted in. If the hypothesis does not lead to certain expected results, it is soon dropped. "If this attempt is correct, *then* I should get such and such results. If I do not get such and such results then I must change my behavior." The 'then,' the anticipated consequences, determines the desirability or goodness of the 'if' behavior. The rat, in the maze or in the discrimination box, *behaves in the very same way* (our emphasis). . . . That the animal's behavior, the 'if,' was conditional and dependent for its continuance upon verification by the goal is demonstrated by the fact that he *changes* his behavior, and tries another response and another until he finally gets one which *is* verified by the efficient attainment of the goal object (10, p. 530).

Recently, the systematic reactions which form the basis of Krechevsky's theorization have been further examined (23, 24). The conclusion provided by these experiments is that the occurrence of these systematic reactions provides no real basis for representing the intelligence of the rat in the extremely favorable light proposed in the above quotation. In fact it is significant that the 'hypotheses' concept assigns to the rat a capacity for problem solving which has been

specifically denied to forms much higher in the psychological scale. Köhler, for example, after extensive investigations of problem-solving behavior in the chimpanzee, excludes the possibility of hypotheses in that form. Thus he writes:

> It is foreign to the chimpanzee, and at its organizational level it is hardly possible for it in such situations to examine in planned fashion, one after another, the various features of the visual field until the correct one is verified by success (8, p. 51).

Considering that the quotations cited above refer to two such widely separated forms, one must conclude that either the one judgment represents an overestimation or the other an underestimation, for the alternative is to represent the rat as psychologically more complex than the ape. On *a priori* grounds the possibility of overestimation is to be favored, for as between overestimation and underestimation of a given organism's psychological capacity, the subject matter of animal psychology is such as to make the former type of error the more likely one. In his researches the animal investigator is largely restricted to overtly manifested behavior. In determining the actual basis of this behavior—that is to say, the meaning it has *for the animal performing it*—much more is left to the observer than is the case at the human level, where subjective checks are available. Further, in interpreting such behavior data it is no easy matter for the investigator to abandon his own familiar frame of reference and to view the situation through the eyes of the animal being studied. To approach the situation with such a higher-level frame of reference means of course to deal with it in psychologically more complex terms. In consequence the danger of anthropomorphism—and hence of overestimation—is a constant one.

We shall see in the course of the present paper that the 'hypotheses' concept provides an instance of such an overestimation of relatively simple behavior data, in consequence of imposing upon these data an interpretation which perhaps fits the observer who obtained them more adequately than the rat which provided them.

II. Characteristics of 'Hypotheses' Behavior

It is desirable at the outset to identify Krechevsky's notion of 'hypotheses' as clearly as possible. Briefly, Krechevsky found that a rat, run in a four-unit linear brightness-discrimination situation, made 'insoluble' through frequent random changes in lights and blinds, tended to adopt a given mode of choice-point response which it repeated at all choice points. With continued training this first response was typically given up for some other response, which was again repeated systematically. In some cases this second response was even replaced by a third one. For Krechevsky each of these responses represents a solution-attempt ('hypothesis'), while a shift from one response to another represents a renewed attempt at solution after the previous attempt had proved unsuccessful.

To distinguish valid 'hypotheses' behavior from descriptively similar reactions, Krechevsky formulated four differentiating characteristics of such behavior: (1) It is systematic. (2) It involves some degree of abstraction. (3) It does not depend entirely upon the immediate environment for its initiation and performance. (4) It is purposivistic, displaying an 'if-then' character. It is worth examining each of these proposed criteria further, first, in order to determine their applicability to the behavior observed, and, second, to determine whether if applicable they warrant the kind of interpretation to which this behavior has been submitted.

1. *It is systematic.*—It has been observed (**10, 23, 24**) that in free-choice and 'insoluble' linear situations the rat will repeat its choices, so that on any given day the same type of response will be made at all choice points. To the extent that this behavior involves the repetition of the same response, and is therefore neither random nor variable, one may say that it is systematic. Actually the term 'systematic' represents no more than a *descriptive* category, and as such may refer to types of behavior ranging from the very simple to the very complex. Thus in these linear situations systematic behavior may represent an organized adjustment to

all four units of the maze as a unified sequence, or it may merely represent a routinized mode of behavior in which the response made in one unit is repeated in other units similar to it. To both extremes the term systematic is applicable, so that the mere fact of systematic behavior is not a sufficient criterion of higher-level processes, as Krechevsky implies in his use of this characterization.

That modes of behavior which are descriptively the same in the sense that they are systematic, may actually have a quite different basis, may be demonstrated with an illustration. A person tapping rhythmically upon a surface may be regarded as showing systematic behavior. But in behaving systematically this individual either may be tapping mechanically or may be beating out a tune. In the first case the individual units of response are more or less independent, and are related only to the extent that they assume the same form. In the latter case the relationship is a more real one, each unit of response having meaning only with regard to the larger series in which it is contained.

That systematic behavior may really have a very simple basis is further shown by its occurrence even among invertebrates. For example, if an insect larva is caused to move clockwise about the confines of a petri dish, and is then placed in a T-maze, it will repeatedly take the clockwise alternative at the choice point (1). Such behavior may be described as systematic, since it is neither random nor variable, and yet it is entirely simple and stereotyped.[1] The same may be said of a rat showing a left-going position habit in a T-maze.[2] In neither case is the behavior organized in the sense that successive responses are inter-related to form a higher-level system.

[1] We may in fact say that stereotyped behavior is generally systematic, and that this feature is an incidental aspect of all behavior which involves the persistent repetition of a given response.

[2] In fact the condition in the linear maze is very similar to that found in the T-maze, for the linear maze consists essentially of a succession of T-units. Where the position habit in the T-maze involves the repetiton of the same type of choice at a *single* junction, the systematic behavior under discussion involves the repetition of a given choice at four T-junctions. In neither case is there an intrinsic relationship among the individual responses, the only connection among them being that they are descriptively the same and are made in fundamentally similar situations.

Thus while we may agree with Krechevsky that the behavior under consideration is systematic, the use of such a designation does not in any way pass judgment upon the complexity of this behavior. In fact, evidence to which we shall refer later demonstrates that this behavior actually has a very simple basis. With respect to the first criterion we may therefore say that it provides no basis for establishing the psychological level of this behavior.

2. *It is abstractive or generalized.*[3]—When an organism makes the same type of response to the common aspect of a series of equivalent situations, we may characterize that behavior as 'generalized' or 'abstractive.' These terms, however, like the term 'systematic,' are essentially descriptive and may therefore also refer to psychological phenomena on very different levels of complexity. In characterizing the behavior under discussion as generalized we are simply describing a state-of-affairs, namely, that the animal responded to a given stimulus at one choice point and made the same type of response to similar stimuli at 39 other opportunities for choice-point response on that day.

Actually the literature provides numerous instances in which generalized behavior was found to have a very simple basis. Köhler (**8**), for example, has demonstrated that a form as low as the chick is capable of generalized responses, so that in a succession of pairs of cards of different brightness it is able to learn to peck from the darker member of each pair. The many experiments on equivalent stimuli have shown that the abstraction of a common element in a series of equivalent situations involves a very simple ability. In fact Maier and Schneirla (**19**), on the basis of a review of the evidence, have concluded that abstraction is not acceptable as a criterion of higher-level processes.

And yet, although the label 'generalized' still leaves open the relative complexity of the behavior to which it is referred, Krechevsky applies it as evidence of a high degree of organization. Thus he writes (**10**, p. 530):

[3] Krechevsky uses the terms interchangeably.

> Only when we have abstracted, from all our data, some general method of consideration of such data can we be said to be working upon an hypothesis. A scientific hypothesis attempts to formulate a general and universal law to give meaning to a large number of special cases.
>
> *The animal's behavior as described above shows that very thing.* (Italic ours.)

To explain the observed generalization of responses in such terms requires additional evidence which does not seem to be available. Actually control experiments by the present writer have demonstrated that such an interpretation is at odds with the evidence, and that an account of this behavior on a simpler basis is vastly more adequate.

It appears that designations such as 'systematic' and 'generalized' represent convenient categories by means of which the experimenter describes certain features which a series of responses, *considered collectively*, may have *for him*. While such designations may serve to facilitate classification, the relationships and organizational features which they introduce are actually imposed by the experimenter upon the obtained behavior data. *As far as the animal is concerned* any true organization among the responses so related need not exist at all.

We may conclude that this second criterion also fails to provide a proper basis for regarding this behavior as higher-level in character.

3. *It does not depend entirely upon the immediate environment for its initiation and performance.*—We can hardly disagree with Krechevsky when he says that these systematic responses represent something of a contribution by the animal, for this must be said in varying degree of all behavior. In fact the present writer (24) has demonstrated with regard to this systematic behavior that there seem to be differences from animal to animal with regard to the kind of response which will appear. Thus one animal will be more apt to repeat left turns, another right turns, one animal light-going turns, another dark-going turns, and so on. These preferences or reaction sets represent characteristics which each animal

brings to the situation and determine the kind of choice which will be made. Such a finding seems to lend support to Krechevsky's argument that these responses are not entirely dependent upon the objective situation for their initiation and performance.

However, the mere fact that the animal's make-up somehow plays a part in determining the form of this behavior does not necessarily prove that it is complex in character. As with the previous criteria, such an attribute may be associated with complex forms of behavior, but since it may also be associated with behavior having a very simple basis, it cannot serve to establish as complex the behavior which it characterizes.

It should be added that when speaking of the animal's 'contribution,' Krechevsky implies a great deal more than is contained in the modest formulation that the animal's make-up enters in determining the form of its response. This is made clear in the following quotation (10, p. 531):

> When we say that an individual has an 'hypothesis' we imply that the individual is contributing something to the situation. His behavior is not something forced upon him by the immediately presented stimuli. He has taken the problem-field and brought to bear upon it all his past experiences. His 'hypothesis' originates in some degree *from within himself.* The situation is *his* situation because it consists both of the external data and of the individual's personal data. An 'hypothesis' is the individual's interpretation of the data, it is not something deriving from the presented data alone.

Actually the above statement greatly overestimates the nature of the animal's contribution to this performance. As has been shown on the basis of some experiments performed by the present writer, the contribution of the animal is not that of formulating solution-attempts in a problem situation, but is simply that of making an initially preferred type of response in a very mechanical kind of situation, which provides no opportunity for a more complex type of adjustment and actually does not permit it. In this respect the contribution of the animal in determining the form of the behavior is

equivalent to that of a rat in a free-choice T-maze when initial preferences are being tested.

Krechevsky writes that the animal brings to bear all of its past experience in formulating the mode of behavior to be followed. It is interesting to inquire into the possible pertinent past experiences which the animal has available. In the studies carried out by the writer in which systematic behavior was observed, young, maze-naïve rats were employed. These rats had been confined to cages all their lives, and it is questionable whether the kind of experience gained under these conditions had any effect upon adjustment to these maze situations. For intance, none of the animals had ever encountered blind-alleys before, and basically it is only such experience which would really be of value in a linear maze problem, requiring for 'solution' blind-alley avoidance.

We may conclude that while these systematic responses necessarily represent, in some measure, a contribution by the animal, this does not prove that these responses involve the higher-level kind of organization suggested by the term 'hypotheses.'

4. *It is purposivistic.*—Among the differentiating characteristics of 'hypotheses' behavior offered by Krechevsky, this fourth characteristic is undoubtedly regarded by him as most important.

When Krechevsky writes that these systematic responses show a purposive, 'if-then' character, he means that the initiation and subsequent performance of this behavior depends directly upon the effect of a goal. Each systematic response represents a solution-attempt, and persists to the extent that it is 'verified by the efficient attainment of the goal object.' In the absence of such verification the first 'solution-attempt' (systematic response) is given up for another, and if necessary for still another. According to this conception the initial occurrence of these responses, and the successive displacement of responses, is goal-determined.

Actually, recent evidence reported by the writer and others indicates that both the genesis and subsequent history of these

responses may be fully explained in terms independent of goal influences. Let us review some of this evidence:

(1) In most of his experiments Krechevsky employed an 'insoluble' linear situation, in the belief that such a situation maintains the animal in a 'problem-solving condition' indefinitely. In the present writer's investigations (**23**) an attempt was made to determine whether the results provided by such an 'insoluble'-situation technic may be referred to situations in which specifically defined learning may occur. To this end 'soluble' and 'insoluble' situations were compared. It was found that whereas animals tested in the latter type of situation invariably developed systematic behavior, animals in the former type as a rule did not show such behavior. This qualitative difference in results suggests that *for the rat* these two situations must be very different. Apparently the 'insoluble'-situation technic upon which Krechevsky largely bases his theorization is not valid for the study of learning. What the 'insoluble' situation provides is not representative learning behavior but a very special type of adjustment peculiar to such situations.

(2) In a series of free-choice situations, which contained no blinds, the writer (**24**) found that every animal showed systematic behavior. If it is argued that these systematic responses are 'hypotheses' how can we explain the occurrence of solution-attempts in a situation which contains no specific problem, and hence does not require any organized problem-solving behavior? In a maze situation the problem which the animal must 'solve' is that of eliminating blind alleys, so that any solution-attempt must be directed at blind-alley avoidance. *Yet systematic behavior was shown by these animals which had never encountered blind alleys, either previously or in the present situation.*

Some of the animals tested in these free-choice situations showed as many as two and three different systematic responses, *and one animal showed four.* Can we validly say that these animals were trying out different solution attempts, when there was no specific problem requiring solution?

(3) Experiments by Snygg (20) provide results which are pertinent in this connection. Snygg worked with a 10-unit linear maze in which all the blinds were located on the right side, so that a left turn was required at every choice point. Snygg found in this situation that *on the very first trial* no animal made an error beyond the seventh unit, and some animals ceased making errors in an earlier unit. Certainly, this behavior, which is systematic, cannot be regarded as goal directed, *for it developed prior to any goal experience.* To the extent that the systematic behavior occurring under these conditions cannot be viewed as solution-attempts, there arises doubt that the systematic behavior shown by Krechevsky's animals represented solution-attempts.[4]

(4) According to Krechevsky the successive displacement of responses, as the genesis of these responses, is to be regarded as goal-determined. Here again, however, experiments support a simpler, non-purposivistic interpretation:

(*a*) In a series of free-choice situations, identical in all respects except that they were graded in terms of the number of stimuli available to the animals at choice points, a direct correspondence was found between the frequency of shift from one response to another and the complexity of the choice-point environment. Tests with a similarly graded series of 'insoluble' situations indicated the same relationship. That is to say, the more heterogeneous the choice-point environment the greater the behavior variability, as reflected in a more ready displacement of responses. The very fact that it was possible to increase the frequency of shift without increasing the difficulty of the problem (and it is doubtful that in the free-choice situations one may speak of a 'problem') suggests that such shifts do not depend in any direct way upon the influence of the goal.

(*b*) The mere fact that *any* displacement of responses occurred in the free-choice situations argues against a purposivistic interpretation, for since the very first response is

[4] Brown and Buel (2) have recently reported the results of a very extensive study of first-trial reactions to linear mazes by human subjects. It is interesting to note that here also systematized response-patterns were observed on the very first trial, prior to any goal experience.

'verified by the efficient attainment of the goal object,' these animals should not have found it necessary to try out other possible solutions. Yet, as we have seen, some animals showed as many as three and four different responses in these free-choice situations.

(*c*) If the displacement of responses is viewed as a renewed attempt at solution, we should expect more frequent displacements in the 'insoluble' type of situation than in the corresponding free-choice type (that is to say, in the free-choice situation with the same choice-point stimulus pattern). Whereas in the free-choice situation the very first response proves efficient, in the 'insoluble' situation this is true neither of the first nor of subsequent responses. Therefore the animal in the 'insoluble' situation should try out a larger variety of responses before finally adopting the one to which it is to adhere. Yet it was found that in corresponding free-choice and 'insoluble' situations the displacement of responses occurred with precisely the same frequency.

(*d*) Finally we may cite experiments by Dashiell (**5**) with checkerboard mazes, in which a series of alternative paths to food was provided. This investigator found that despite the equal directness of all paths his animals shifted spontaneously from one route to another in proceeding from start to food box. It would seem improbable that these shifts in behavior were related in any direct way to the influence of the goal, or that each successive shift represented a new solution attempt. Rather as with our linear mazes, a shift from one pattern of response to another seems to reflect the type of variability which occurs in a heterogeneous situation that provides many alternative modes of behavior.

Such variability for the sake of variability alone has been observed in many instances of animal behavior (**25, 7**). The present writer, for example, has found systematized responses in a one-unit free-choice situation consisting of a single choice point, preceded by a U-turn and followed by two equivalent paths to food. Further, *though both alternatives led to food*, the typical animal shifted from one systematic response to another. The facts that such shifts occurred under the condi-

tions provided by this one-unit situation indicates again that these shifts are not goal determined.

The evidence just reviewed militates consistently against a purposivistic interpretation of the systematic behavior under discussion. Such an interpretation imposes upon these relatively simple behavior data a complex theoretical structure which is actually at variance with the facts, and elevates the psychological level of this behavior in a very unwarranted way. We may conclude from our examination of Krechevsky's fourth criterion, as we did with the first three, that there is no basis for representing this systematic behavior as 'hypotheses' formation.

Whereas the interpretation developed by Krechevsky is inadequate at many points, there is a simpler, non-purposivistic explanation which seems far more consistent with the evidence. This alternative explanation is presented below.

Considering first the genesis of systematic behavior, we find it significant that it invariably occurs in free-choice and 'insoluble' situations, but is typically absent from 'soluble' situations. Such a finding suggests that systematic behavior can occur in linear situations only when the opportunity for organized and specifically defined learning is absent. The basis for this statement becomes clear when we consider the essential nature of each of these situations. Since in a 'soluble' linear situation specific learning at each choice point is possible, the animal may select and fixate a specific sequence of choice-point responses in attaining the goal. In the 'insoluble' type of linear situation continued random changes in the features which might form the basis of a learned pattern of response make the selection and fixation of any particular sequence of responses *impossible*. Finally, in the *free-choice* type of situation, where at each choice point both alternatives lead to food with equal directness, specific learning is quite *unnecessary* and getting to food does not depend upon the selection and fixation of any particular path. Thus, in the 'soluble' type of situation differential choice-point behavior (*i.e.*, making at each choice point the response which is correct at that choice point) permits the animal to get

to food in most direct fashion. Consequently in this type of situation systematic behavior does not occur. In the free-choice and 'insoluble' situations, on the other hand, making a different choice at each choice point proves to be no more direct than repeating the same choice.

Though both of these possibilities are available, actually the repetition of choices (systematic behavior) is invariably the rule. That the latter occurs depends upon certain further special features of design and technic: (1) Since the maze is linear in character, the units in which the choices are to be made are structurally identical. (2) The choices must be made in rapid succession. (3) A large number of choices is required. When confronted with the same situation time after time, under conditions where one type of choice proves to be no more direct than any other, the animal hurrying to food adjusts by repeating the same response over and over again. In other words we may account for the genesis of systematic behavior in certain types of linear situation with the simple fact that a mechanical, serialized type of situation elicits a mechanical, serialized type of adjustment.

An account of the displacement of responses which follows the adoption of an initial response may be given in equally simple, non-purposivistic terms. In these free-choice and 'insoluble' patterns opportunity is provided for various alternative responses, but there is nothing in the situation to encourage either the selection of one response over another, or persistence in the first response adopted. In such a heterogeneous situation, which provides stimuli which may form the basis for many different responses, the rat is very apt to respond to different aspects of its environment and change its behavior accordingly. The displacement of responses thus reflects the simple fact that as a change occurs in what the animal finds perceptually prominent in the situation, there is a corresponding change in its behavior, and that the more there is to attend to the more frequent are the behavior changes. This interpretation is supported by the finding that in a series of free-choice and 'insoluble' situations, an increase in the heterogeneity of the choice-point environ-

ment produced an increase in the number of different kinds of responses made.

This conception of the genesis and subsequent development of systematic behavior in certain types of linear situations, formulated in simple, non-purposivistic terms, is offered as an alternative to Krechevsky's 'hypotheses' conception.

III. A Reevaluation of Krechevsky's 'Hypotheses' Experiments.

In the course of his investigations into the nature of 'hypotheses' behavior Krechevsky performed a number of experiments. It will be profitable to consider at this juncture the results of these experiments in light of the 'hypotheses' conception and the alternative conception advanced by the present writer.

(1) The conception which we have developed in the present series of papers is based in part on the finding that, though occurring in 'insoluble' situations, systematic behavior is typically absent from 'soluble' situations. In an early experiment by Krechevsky (9), however, systematic behavior was observed in a 'soluble' situation. Does this finding contradict our interpretation which admits systematic behavior only in situations which exclude the possibility of specific choice-point learning? Or may these findings be explained in a manner consistent with the interpretation we have proposed? [5,6]

[5] The findings of systematic behavior in 'soluble' situations has specific significance from the standpoint of experimental procedure. The argument may be raised that the 'soluble' situations we employed, in which no systematic behavior appeared, were sufficiently easy to permit direct mastery, without any preliminary attempts at solution (*i.e.*, without 'hypotheses'). It might therefore be argued that since these situations were so easy, a lower order of behavior proved adequate, and hence, these situations did not constitute a fair test for the animal. If we are really to give the animal an opportunity to display its ingenuity via 'hypotheses,' it is necessary to employ more difficult problems. To use an analogy: If a very simple arithmetic test is presented to a mathematics professor and to a second-grade elementary school student, both may do equally well. If the presumed difference in ability is to be tested validly, a more difficult examination must be provided.

As we shall see, however, this argument has no real validity. Actually it is not the fact of greater difficulty, *per se*, which accounts for the occurrence of systematic behavior in certain types of linear 'soluble' situations. Rather is it the case that

Let us consider Krechevsky's experiment. A group of 40 animals was run upon a linear hurdle-discrimination problem. At the entrance to each correct alley was inserted a rod, or hurdle, over which the animal had to jump. In this learning situation Krechevsky found that some animals showed a systematic repetition of choices prior to the attainment of the learning criterion.

An examination of these results shows that *only 15* of the 40 animals tested clearly showed systematic behavior. Furthermore, the animals which displayed systematic behavior were the ones which experienced great difficulty in learning. Now one may ask why it is that animals slow to master the problem show systematic behavior prior to the attainment of learning criterion, while the rapid learners do not. The answer to this may be found in a comparison of the learning curves for good learners and poor learners presented by Krechevsky.

From these curves it is clear that among animals which do not show systematic behavior there is an immediate decline in errors. That is to say, such animals apparently begin to learn about the features essential for the solution *almost from the very outset.* The curves for the members of the non-systematic behavior group, on the other hand, do not show this immediate decline, but continue at the initial level for many trials. One may conclude that as long as the error curve continues at such a constant level, the animal has not begun to respond to those features of the problem which the experimenter has made important for solution. In the case of a typical member of this group, for example, the drop does not take place until after the seventieth trial, which fact indicates that this animal received about 70 runs through the maze without learning about these crucial features. Since during that time the blinds and hurdles were changed after

some 'soluble' situations may under certain circumstances be rendered effectively 'insoluble,' eliciting the mode of adjustment typically associated with the latter type of situations.

[6] In one of the 'soluble' situations employed by the present writer (23) an instance of systematic behavior was observed. The explanation offered below to account for the occurrence of systematic behavior in Krechevsky's 'soluble' situation may be applied to this case as well.

each trial, and since for this animal there evidently was no effective relationship between a change in blinds and a change in hurdles, this situation was really equivalent to an 'insoluble' one. And to such an 'insoluble' situation it reacted just as rats typically respond to any linear 'insoluble' situation—*i.e.*, by a simple repetition of choices.

Systematic behavior in this particular 'soluble' situation is thus restricted to slower learners who for extended periods fail to respond to the available relationship between hurdles and blinds, so that when these features are changed in what for the animal is a random order, the situation becomes to all intents and purposes an 'insoluble' one. The mere fact that a solution exists here *for the experimenter* does not make the situation different during this period from a truly 'insoluble' one in which a solution exists neither for the experimenter nor for the rat.

How are we to account for the lengthy period which elapsed before the systematic-behavior animals began to respond to the hurdles in their essential relations? The real cause seems to lie in the rather unobstrusive character of the relationship to be learned. In this respect Krechevsky's hurdle-discrimination situation contrasts with the 'soluble' situations employed by the present writer, where the speedier rate of mastery indicates a greater ease in perceiving the relation to be learned.

Brief reference to the problem-box type of situation will perhaps make clearer the basis of the learning curve provided by the systematic-behavior animals in the hurdles situation. In the problem box too, the learning curve typically shows an initial plateau followed by a fairly sudden drop. The basis of this sequence is well established. It lies in the fact that the relationship to be learned by the animal is also frequently unobstrusive in character. This is the case, for example, when the set-up devised by the experimenter is an arbitrary one, in the sense that there is no intrinsic relation between the food and the response required to obtain it. An instance of such a set-up is provided by the type of situation in which the animal must step on two platforms in given sequence to

open a door which leads to food. The relationship may also be regarded as unobstrusive when the cue which must be used is relatively hidden or for some other reason is difficult to perceive. In all such situations the animal frequently spends much time in attending to certain extraneous features of the situation. Thus, it may dig in the sawdust, gnaw at the screen and so on. Though these extraneous activities enable the animal to learn more about the problem box as a whole, they contribute little if anything to the mastery of the problem *as set for the animal by the experimenter.* In other words, in these situations, *effective* learning (that is to say, actually useful fixation) does not begin until the animal commences to respond selectively to features essential for its solution. The more unobtrusive these features are the longer is the solution delayed. Further, animals which delay in responding to these features experience undue difficulty with the problem.[7]

The behavior of the animals which showed systematic responses in Krechevsky's hurdle problem may be similarly interpreted. These animals failed also to respond to features essential for the solution of the problem until a late point, and prior to that point they too engaged in extraneous behavior. Thus, they responded in terms of the kinesthetic impulses which resulted from taking the *left* alleys, then perhaps in terms of the impulses which resulted from taking the *right* alleys and so on. Because of certain features of design and technic, they responded at any given time in the same way at all choice points, to the end that their behavior was systematic. Here again such behavior contributed little if anything to the mastery of the problem the experimenter had prepared. One may say in fact, that these responses to certain spatial features of the situation have as little to do with mastery of this problem as has digging in the sawdust or gnawing at the screen to do with mastery of the problem box which requires the animal to step on two platforms in

[7] This interpretation of problem-box behavior is consistent with the analysis offered by Maier and Schneirla (19, Chapter 16). The learning theory of the same writers advances the distinction between fixation and selection employed here.

appropriate sequence. Further, in both instances the form of the behavior is determined essentially by local features of the situation, and a change in behavior depends upon a shift in what the animal finds perceptually prominent.

We have accounted for the occurrence of systematic behavior in *difficult* 'soluble' situations in terms of the delay in selection of certain essential features of the situation. It is necessary in this connection to distinguish between two general types of difficult learning situations. Two situations may provide learning curves which take equally long to reach the base line, but where one curve continues on a constant level and then drops precipitously, the other shows a more even and gradual decline. Though by objective learning criteria both situations are regarded as equally difficult, it is obvious that the basis of difficulty is different in each case. In the instance where the curve shows a sharp drop after a period of no improvement, the difficulty results from a delay in perceiving certain required items or relationships. In the case of the curve which shows a more steady decline, on the other hand, the difficulty is not so much in determining what is to be learned (selection), as it is in the actual occurrence of learning (fixation).

The difficulty in Krechevsky's hurdles situation seems to have been of the first kind, and we may venture the prediction that only in a difficult 'soluble' linear situation where the difficulty is specifically of this character will systematic behavior occur. The basis of this prediction is readily explained. Where the difficulty is mainly related to selection, the changes made in the situation are for a time unrelated for the animal, and so long as this continues to be the case, specific learning about each choice point cannot take place. This is the case, for example, in Krechevsky's hurdles situation where prior to mastery of the relation between hurdles and blinds there can be no effective learning about each choice point. Under these conditions systematic behavior may occur, for reasons previously considered. Where the difficulty is related to fixation, on the other hand, the animal usually learns very early that there are differences between

alternatives as to their directness in reaching food. It is mainly the specific character of these differences that is difficult to master. This is the case, for example, when sensory cues which may serve to differentiate the alternatives at choice points are not directly available. Under these circumstances the animal soon begins to behave differently from choice point to choice point, so that systematic behavior is ruled out. It is for this reason that we predict that to the extent that systematic behavior occurs at all in difficult 'soluble' situations it will occur only when the difficulty is related to selection.

This discussion indicates that the occasional occurrence of systematic behavior in linear 'soluble' situations does not contradict our original conclusion that 'soluble' and 'insoluble' situations are basically different for the animal. The conception previously presented to account for the occurrence of systematic behavior requires no broadening to explain the finding of such behavior in certain types of 'soluble' situations. As we have attempted to demonstrate, in such instances, the 'soluble' situation has for the animal the essential characteristics of an 'insoluble' situation and elicits the behavior typically associated with that type of arrangement.

(2) In another experiment Krechevsky (11) investigated the 'docile' character of 'hypotheses.' Docility is defined by Tolman (21, p. 443) in the following terms:

> When a behavior act *A* is docile, it will give way . . . to a subsequent act *B* as a result of any major change in the environmental situation. The new act *B* will conform to the new environmental situation (get to the demanded type of goal-object), in a way that *A* did not.

According to Krechevsky if 'hypotheses' show docility, a given 'hypothesis' should be given up quite readily if a different one is made more efficient. To test the docility of 'hypotheses' Krechevsky performed the following experiment: Each of a group of 60 animals was tested upon an 'insoluble' situation until it began to repeat some choice systematically. The animals were then divided into three

groups: *Group I:* Following the adoption of a given systematic response on the 'insoluble' situation, the situation was made 'soluble,' but in such a way that a different response than the one being followed became correct. Thus, if an animal of this group showed a systematic *light-going* response, the situation was changed on the next day to make all of the *darkened* alleys correct. *Group II:* After the members of this group had shown their first systematic response on the 'insoluble' situation, the situation was also rendered 'soluble,' but now this first response was made correct. Thus, if an animal showed a left-going response, the situation was changed to make all of the left alternatives correct. *Group III:* This group served as a control in that the situation was allowed to continue 'insoluble.'

The results showed that (*a*) The animals of group I tended to give up the systematic response which had been followed on the 'insoluble' situation and to adopt the one which had been made correct. (*b*) The animals of group II showed an increase in the frequency with which they followed the systematic response adopted on the 'insoluble' situation, when that response was made correct. (*c*) The control group continued to adhere, more or less, to the first response adopted. Since the systematic responses were given up rather readily when some other response became more effective, and were adhered to more consistently when they themselves were made efficient, Krechevsky concluded that 'hypotheses' show docility.

Let us examine the basis of these results by considering a few hypothetical cases. On the first day of testing, a given animal in the 'insoluble' situation begins to repeat its *left* choices from choice point to choice point and from trial to trial. Since the situation is 'insoluble' this left-going response is no more direct than any other response. Now on day 4 the situation is made 'soluble' so that at all four choice points the *right* alternative becomes correct. Promptly the number of left-going choices made by the animal decreases, and the number of right-going choices increases. But could one expect a different outcome?

The results of an experiment we performed furnish pertinent evidence on this point. A 'soluble' maze of the usual linear design was employed, but the blinds were changed from day to day so that a different pattern was correct each day. It was found that when the arrangement was such that the *same* alternative was made correct at *every* choice point, the set-up was mastered very readily—in fact, in just a few trials. Now when Krechevsky made his 'insoluble' situation 'soluble,' in every instance the changes were such as to make the same response correct at all choice points. Surely it is permissible to assume that without 'hypotheses' enough could be learned about this truly simple problem so that by the end of the 10 trials allotted daily the great majority of choices would already be correct. This would explain why the animals of group I gave up the systematic response adopted on the 'insoluble' situation when some other response was made correct. It would further explain why the response shifted to was the one which had been made correct.

The results for group II may be interpreted in the same way. One may illustrate this with the hypothetical case of an animal which is making left-going choices on the 'insoluble' situation. Let us assume that the day after this left-going response is repeated to the point where its repetition could not be attributed to chance alone, all of the left alternatives are made correct. Even a completely naive animal should be able to make predominantly correct choices in such a situation within a few trials on the basis of very simple learning. But learning is also greatly expedited by the fact that the animal had been taking the left alternatives even before they were made correct.

Thus, the results of these experiments permit no conclusion beyond the fact that the rat is capable of learning simple problems. Hence it is not only unnecessary, but it is actually unwarranted to introduce such concepts as 'hypotheses' formation to explain these results.

(3) In one series of experiments Krechevsky (**12**) ran some 'maze-dull' and 'maze-bright' rats from the selected stock of Tryon (**22**), and compared their behavior with a group

of controls. It was found that the maze brights preferred spatial responses and the maze dulls visual responses. The controls showed both types of response with equal frequency.[8] From these results Krechevsky concluded that the kind of 'hypothesis' adopted by any rat is to some extent a function of the heredity of the animal.

The factor which Tryon employed in distinguishing brights from dulls was performance in an alley maze. Now it is known that the alley-maze habit does not involve vision in any important way, being based mainly upon 'spatial abilities,' such as kinesthesis (4, 6). This fact would lead us to expect that the differentiation between dulls and brights is primarily in terms of such 'spatial ability,' with the brights superior in this respect. That it is essentially a matter of the brights preferring spatial responses, and the dulls simply *not preferring* such responses (rather than positively preferring visual responses) is indicated by Krechevsky's finding that 69.2 per cent of the responses shown by the brights were spatial while only 50 per cent of the responses shown by the dulls were visual. Thus while the brights showed a marked preference for spatial responses, the dulls adopted visual responses only to the extent of chance. Further, in the brights, 69.0 per cent of the individual choice-point responses were made in conformance with some visual habit and 83.8 per cent in conformance with some spatial habit, while in the case of the dulls, 75.8 per cent of the responses followed a visual habit and 70.6 per cent followed a spatial habit. By this criterion too the preference of brights for spatial responses is marked, while the dulls showed spatial and visual responses with above equal frequency. It appears then that the real distinction here is between spatial and non-spatial animals, rather than between spatial and visual.

The results of this experiment indicate that features of structural organization do play a part in determining the character of these systematic responses, but as concerns the

[8] In a *representative* group, it is not the case that some rats are spatial (prefer spatial responses) and others visual. The writer found that in situations where both visual and spatial stimuli were available, the *same* rat would show visual responses at one time and spatial responses at another.

possibility that these responses are 'hypotheses' they prove nothing. The existence of a structurally determined preference for the dark or for left turns, for example, does not in any way establish the responses to which these preferences lead as higher-level in nature. We have indicated previously that the involvement of features in the animal's make-up in determining the form of a given response does not in itself set the psychological level of that response. It is on this basis that we conclude that this experiment provides no evidence to prove that these systematic responses are 'hypotheses.'

It should be noted that the results of this experiment provide specific support for our previous suggestion that the preference demonstrated in the adoption of one systematic response rather than another may depend upon relatively enduring features in the animal's make-up. Such features, however, seem to determine the *form* of a given systematic response (as whether it will be dark-going or light-going, for example), but do not seem to enter in accounting for the *occurrence* of such responses.

(4) In another series of experiments Krechevsky (13) attempted to determine the effect of decortication upon systematic behavior. In the 'insoluble' situation, he found that as compared with normals, decorticates showed less frequent shifts from one systematic response to another. This reduction in variability was indicated by the fact that the representative animal showed fewer systematic responses and adhered to each response for a longer period.

But such a finding may be readily explained without recourse to an 'hypotheses' interpretation. There is much evidence to indicate that a decorticate animal is a more stereotyped animal (3, 17, 18, 14, 15, 16). A decorticate animal is less affected by changes in its environment, and is relatively inattentive to parts of the situation other than that to which it is attending. Thus a decorticate animal responding in terms of the kinesthetic impulses resulting from its movement through a linear maze would tend to be less alert to the visual features of the maze. The net result is to decrease the number of stimuli which may be effective in determining

that animal's behavior at each choice point. Such an account, given in terms of these well-known facts regarding the effects of decortication would explain Krechevsky's results very adequately.[9]

Certainly these results do not require that we assume the loss of a very complex ability, nor do they provide any proof that these systematic responses are 'hypotheses.' In fact these decortication experiments contain evidence attesting to a very simple basis for systematic behavior. This evidence is to be found in the fact that *every one of Krechevsky's operated animals showed systematic behavior.* Surely, if these responses represented 'hypotheses,' we would expect that at least a few animals (especially those suffering severe cortical lesion) would fail to show systematic behavior. And yet not even among animals with extensive lesions did this behavior fail to appear. Lashley (17) and others have found that an extensive destruction of the cortex caused some rats to fail in the learning of even very simple mazes, and Maier (18) has reported a complete absence of 'reasoning' in a number of decorticate animals. Surely, if this behavior were of the complex character claimed by Krechevsky (and according to his definition it is more complex than the kind of ability involved in learning a simple maze or in solving one of Maier's 'reasoning' problems), it should be completely lost in at least a few extreme cases of decortication. The fact that systematic behavior appeared in every operated animal points to its very simple basis.

The observation that decortication leads to reduced variability may be correlated with the present writer's finding that a decrease in variability may be accomplished through decreasing the complexity of the stimulus situation at each

[9] Krechevsky (13) reports that destruction of at least 20 per cent of a given cortical area (area '*S*') and less than 20 per cent of another area (area '*V*') results in a preference for visual responses. Conversely, destruction of more than 20 per cent of area *V*, regardless of the amount of destruction in area *S*, results in a preference for spatial responses. The indicated relationship between the area destroyed and the behavior affected suggests a possible interference with sensory tracts. If such interference did occur it would decrease the availability of stimuli for choice-point orientation as effectively as if the stimuli were removed from the environment. This may account in part at least for the less ready displacement of responses found in decorticates.

choice point. Taken together these two lines of evidence lead to the conclusion that a reduction in behavior variability may be accomplished either by simplifying the animal or by simplifying the environment. In the latter case we are reducing the number of stimuli to which the animal may respond, in the former we are reducing the responsiveness of the animal to stimuli which are available. Both accomplish the same end—more persistent attention to a smaller number of stimuli, or reduced variability.

Thus we find that the results of each of Krechevsky's experiments conform satisfactorily with the simple, non-purposivistic conception we have offered to account for the systematic behavior described.

IV. Summary and Conclusions

Many facts concerning the rat's adjustment to various types of linear 'insoluble' situations, accumulated mainly in the course of experiments by Krechevsky and the present writer, are now available. The 'hypothesis' theory and the theory of the present writer represent alternative interpretations of these facts. In the present paper we have reviewed the data, which both conceptions must accommodate, and we have considered the ability of each of these conceptions to explain these data. It was found, in the course of this analysis, that a number of items of evidence failed to fit in with the 'hypotheses' concept. We list below some of the more significant of these items, together with the reasons for their incompatability with the 'hypothesis' interpretation:

1. The fact that systematic behavior is typically absent from linear 'soluble' situations, though present in 'insoluble' and free-choice situations, indicates that such behavior may not be taken as characterizing the rat's adjustment to learning situations.

2. The presence of systematic behavior in linear free-choice situations shows that these systematic responses cannot be solution-attempts ('hypotheses'), for these free-choice situations do not contain a problem which requires solution

in the sense of demanding the selection and fixation of a specific response-pattern.

3. That in every instance of systematic behavior the repetition of the very same type of choice-point response was involved demonstrates that this behavior does not represent an adjustment to all four units as an organized sequence but rather that it is the individual unit of response which is important. Were we dealing with an adjustment to a unified series, we would expect RRRL or RLLR 'hypotheses,' for example, as frequently as LLLL or RRRR 'hypotheses.' After all, why should the animal 'consider' only that kind of 'hypothesis' in which the same response is correct at all choice points?

4. In 'hypotheses' terms the first 'hypothesis' adopted on the free-choice situation is satisfactory in that it permits the achievement of the goal without any encounters with detours or obstacles. In light of the effectiveness of the first 'hypothesis,' why should the animal proceed to 'try out' a second, a third, or even a fourth 'hypothesis'?

5. In a free-choice situation the very first 'hypothesis' proves efficient, while in the 'insoluble' situation no 'hypothesis' meets this criterion. Therefore one would expect that in the former type of situation the animal would give up 'trying out' new 'hypotheses' sooner than in the latter type. Actually displacement of responses occurs with equal frequency in both situations.

6. The day-to-day curve for new systematic responses starts out at a peak and drops progressively from day to day. It is far easier to explain the readiness with which a naive animal adopts systematic responses if these responses are regarded as simple routinized adjustments, rather than 'hypotheses.'

7. The fact that in long linear mazes a systematic repetition of choices is demonstrated on the very first trial indicates that the adoption of these responses cannot be directly related to food-getting.

8. Habits having a far simpler basis than 'hypotheses' cannot be performed after extensive cortical lesion. Yet

every animal submitted to decortication showed systematic behavior in linear 'insoluble' situations, pointing to the relatively simple basis of these responses.

Where the 'hypotheses' concept has been unable to accommodate a number of important facts, the simpler, non-purposivistic interpretation of systematic behavior developed in the present series of papers has successfully accounted for—and in fact has grown out of—these and the other facts on hand. To the extent that the test of any theory lies in its ability to cope with *all* of the pertinent evidence, we feel justified in offering our non-purposivistic interpretation of systematic behavior to replace the 'hypotheses' conception.

There is a final point to make. The 'hypotheses' concept and the system of which it is a part arose in opposition to the crudely mechanistic conceptions which have gained much prominence in the field of learning. According to the latter theories, learning is essentially a matter of establishing connections between stimuli and responses, with the animal serving mainly as a recipient of stimuli and a maker of responses, but playing no very dynamic part in determining which response is appropriate to which stimulus. Further, in such views the beginning of the learning process is haphazard with each response-unit imbedded initially in a complex of random activity. As opposed to such piecemeal conceptions, the 'hypotheses' concept attempted to present the learning process as orderly, organized, and predictable, and pictured the learner as a more active participant in determining the character of the final learned habit. This, in our estimate, is a very desirable end, but since the specific implementation of this intention in the 'hypotheses' concept swings to an extreme where it is beyond the bounds of the evidence, it constitutes a very weak kind of opposition to the elementarism it attacks.

Certainly the rejection of the 'hypotheses' concept does not mean a return to the crude atomism in learning theory which it was invoked to destroy. A purposivism or a crude mechanism are not the only theoretical alternatives available.

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THE NATURE AND EXPLANATION OF BEHAVIOR

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The progress of a science is marked by a growing certainty as to subject-matter, and a clearer conception of the nature of that subject matter. Psychologists are agreed that the subject-matter of psychology is the behavior of the human organism, but they do not share a common conception of the nature of behavior, and for that reason they differ about what it is that psychology is called upon to explain. Indeed, it is likely that it is disagreement upon what are the data of psychological science that is the primary source of the divergence between the 'schools' of psychology. It would be surprising if failure to agree upon what data are to be explained were followed by anything but widely divergent theories.

The two most commonly held (and widely divergent) conceptions of behavior are, on the one hand, that behavior consists of the movements of the bodily members in space, and on the other hand, that behavior consists not of the bodily movements *per se*, but of the effects of the movements. On the first view the behavior of a man running a race would be taken to be the movements which he makes in running. On the second view, however, his behavior would be taken to consist of competitive activity, self assertive activity or some such mode of conduct. In this case the bodily movements of running are taken to have the effect of asserting the individual's superiority. Murray (**6**) has applied the name 'dynamicists' to those who view behavior as the effects of movements, while the conception of behavior as bodily movements *per se* has been most definitely associated with the name of Watson, although, as we shall see, he did not adhere to it consistently. In the following discussion the terms 'dynamicist' and 'Watsonian' will be used with this connotation.

At this point it is necessary to meet an objection, which, if sound, would invalidate the whole argument. It might be objected that when the dynamicists refer to 'needs' or 'drives,' the reference is not to the effects of bodily movements, but to their causes. Such an objection, however, springs from a misconception of the nature of a need or drive. Unless it is argued that we can have direct knowledge of other people's impulses to action, it must be admitted that needs and drives are conceptional constructions, not data. To say that the human organism has a hunger drive is merely to say that whenever the organism remains without food for a number of hours, it alters its spatial relationship with the environment, and continues to do so until food is ingested. A need or drive is simply a name for an observed uniformity of behavior, the uniformity in question consisting in the fact that the organism's bodily movements produce a particular effect. To say that a given action is an expression of a need for food is simply to say that it tends to produce the effect of ingesting food. (Whether the organism consciously intended to ingest food is another question, which must be decided on other grounds.) To describe (or explain) bodily movements in terms of needs or drives is, therefore, to describe (or explain) them in terms of their effects.

Many a bitter conflict in psychology may be traced to the clash of the conception of behavior as bodily movements, with the conception of behavior as the effects of these movements. The controversy over the concept of 'instinct,' for instance, arose mainly because the disputants were trying to explain different data, and naturally were forced to use different concepts to do so. Watson and his followers succeeded in demonstrating that the innate *reflex patterns* of man are relatively unimportant. McDougall and other dynamicists replied that Watsonians had not disproved the existence of innate *needs* or *drives* which merely utilize the reflex patterns as means of achieving their ends. Few of the participants in the debate recognized that the basic source of the dispute was a difference of opinion as to what are the data of psychology, and each side tended to charge the other with failing to

explain the data with which they themselves were primarily concerned. The dynamicists accused Watson of neglecting the important data of psychology; the Watsonians retorted that the dynamicists failed to explain bodily movements. The issue is considered in a later section of this paper. The present intention is merely to point out that the conflict over instinct was at bottom a conflict over what are to be considered the data of psychology.

The battle over instinct no longer rages; other theoretical problems are now the center of interest, but the fundamental problem, what are the data to be explained by psychology, has received little attention, except from those who have been concerned to mark off the field of psychology from the domain of other social sciences. It has not been sufficiently realized that psychologists who agree on the demarcation of psychology from other sciences may yet have profoundly different conceptions of the nature of its subject-matter. Both the Watsonians and the dynamicists would agree, for instance, that psychology studies the behavior of individuals, whereas sociology studies the conduct of groups, but their conceptions of what is behavior are very different.

The reason for such divergence between opposing conceptions of behavior lies in the nature of behavior itself. *Behavior consists simply of changes in the relationships between the organism and a given system of variables, of a shift in the position of the organism in a given field or context.* At any given instant the organism is in several different contexts at once, and behavior may be occurring in them all. The divergent 'schools' simply observe the organism in one context, and neglect the others. To be accurate, we should say that psychology is the science of human behaviors since there is no one behavior, but as many behaviors as there are distinguishable contexts.

"To study the behavior of the bee," writes Holt (2, pp. 77–8), "is of course to put the question, What is the bee doing?" Similarly, to study the behavior of the human organism is to put the question, "What is the man doing?" If we ask this question concerning Roback's (7, p. 84) ex-

ample of an acrobat performing a balancing stunt on the stage of a crowded theatre, various answers may be made. If we have the same conception of behavior as the dynamicists, our reply might be that the man is performing an acrobatic stunt. This answer would hardly satisfy most dynamicists, however, and they would rush forward with 'more basic' aspects of the acrobat's actions, such as the fact that he is amusing the audience, or earning a living, or realizing an ambition. Writers of McDougall's persuasion would proceed to attribute his earning a living to an instinct for nutrition, his amusing the audience to an instinct for display, and his realizing an ambition to his instinct for self-assertion or his sentiment of self-regard. Even more die-hard instinct-theorists would say that the man's actions are an expression of his will-to-power, or his libido, or his instinct for self-preservation.

On the other hand Watson and Holt would be satisfied by the answer that he is performing an acrobatic stunt, although if pressed, they would proceed to reduce the acrobatic stunt to certain complex co-ordinations of reflexes, and would explain its occurrence in these terms.

It appears, then, that there is no single answer to the question: *What is the man doing?* At the one time he is doing as many things as there are contexts to which he is related. Once we begin to consider the *effects* of bodily movements there is no knowing where to stop. Any bodily movement produces a number of effects, none of which is logically prior to the others. Putting food in the mouth (and thus in the stomach) has the effect of relieving hunger pangs, but it also has the effect of preserving the individual, and through him the race. Are we to consider this behavior as simply putting food in the mouth, as a manifestation of the hunger drive, as an effort to restore homeostasis, or as an expression of the drive to self-preservation? Any bodily movement may be *formally* reduced in this fashion to a manifestation of a number of needs or drives, and if the reduction is pushed *ad absurdum*, to the drive for the preservation of the individual and the race. It is these various interpretations

of the effects of bodily movements (all equally valid) that are responsible for the division in the dynamicists' camp, and for the bewildering disagreements between the various lists of basic drives.

The error of the dynamicists lies in their tendency to build up, on purely *formal* grounds, hierarchies of drives, all the drives lower in the hierarchy being regarded as merely a particular expression of the drives higher up, which are held to be the real, basic drives of the human organism, to which, in the last analysis, all behavior may be attributed. Such a hierarchy is obtained simply by viewing the behaving organism in relation to a number of different contexts. When seen in relation to its physical or social survival, the organism's eating behavior is a manifestation of its drive to survive. When seen in relation to the ingestion of food and the physicochemical changes consequent upon this process, it is relieving its need for food. Seen in spatial relation to the geographical environment, it is making spatial adjustments.

On these grounds, there is no reason to call any of these drives more *basic* than any other. We may, if we desire, regard all behavior as the expression of a drive for self-preservation, but such a procedure, although formally valid, does not aid us in the explanation of any behavior other than that of preserving the individual. We can infer preservation of the individual from eating behavior, but we cannot infer eating behavior from preservation of the individual.[1] The occurrence of eating behavior, rather than some other form of self-preservative activity, still awaits explanation.

The point is that in calling the drive to self-preservation more fundamental than the hunger drive, for instance, and in saying that the hunger drive is nothing but an expression of the organism's fundamental urge to preserve its integrity as a unit, we are trying to explain events in one universe of discourse by events in another. Another example may indicate the nature of the error more clearly. Suppose a man

[1] Stating the argument in logical form, we have: "All instances of eating are instances of self-preservation." (All A are B.) Obviously, it does not follow that "all instances of self-preservation are instances of eating." (All B are A.)

eats frogs' legs for lunch. We could regard the man in relation to the ingestion of food and the processes of digestion, etc., and say that his behavior is an expression of his hunger drive. The statement that he eats because he has a hunger drive, however, does not explain why he eats frogs' legs. To explain that behavior, different concepts (involving learning), must be employed.

Failure to recognize that events in one universe of discourse cannot be explained by concepts referring to events in another universe of discourse is responsible for many acrimonious debates in psychology. Watson charges the dynamicists with not explaining the organism's bodily movements. The charge is just; the concepts of the dynamicists do not explain bodily movements *per se*, for the simple reason that they were never intended to do so. To the dynamicists, the acrobat's behavior does not consist of bodily movements; it consists of earning a living, amusing an audience, realizing an ambition, satisfying his drive for self-assertion, and so on. The dynamicists argue, in counterattack on Watson, that these behaviors are not to be explained by reducing them to the bodily movements involved in them.

It is true that the bodily movements are not to be explained by reducing them to manifestations of certain drives by the simple process of pointing out that they achieve certain effects. The one effect may be achieved in various ways. The occurrence of the effect does not explain why it was achieved in a particular way. It is also true that the effects of the bodily movements are not to be explained by the occurrence of the movements. The acrobat's movements have the effect of earning him a living, but we cannot infer from the fact that he makes these movements that he earns a living, or even that he amuses the audience. There may be no audience there, but the bodily movements may occur nevertheless.

This brings out the confusion which runs through Watson's whole theory of behavior. Watson tries to have it both ways. For Watson, behavior consists of a stimulus and a response, but as Bentley (1) has pointed out, he defines these categories so widely as to render them useless for science. Watson

includes in his category of stimulus anything from a flash of light, a prick, a loud bang, or the chemical effect of a hormone in the blood-stream, to a conversation, a table, or a newspaper, and in doing so, he moves freely from one universe of discourse to another without even realizing it. Clearly, a newspaper may be regarded as a black and white object or as something that may be read. A loud bang may also be a gun-shot, or a door swinging to. Watson mixes the environment described in terms of its purely physical properties with the same environment described in other terms. The confusion appears also in his category of response, which includes anything from a knee-jerk to a declaration of war. Here we have the organism's relationship with an environment of purely physical energies confused with its relationship with an environment characterized in terms other than physical.

If Watson characterizes the stimuli impinging upon the organism in purely physical terms, he must characterize the responses of the organism in the same terms. A prick followed by a shift of the pricked limb—this is the type of event which Watson is called upon to explain, and no other. If, on the other hand, he sets out to explain events such as the declaration of war, he must do so in terms other than the physical stimuli impinging upon the statesmen in question. In the first case the data to be explained are muscle-twitches; in the second case the data are the effects of the muscle-twitches upon the political situation. The major contribution of Tolman (**8**) is to have seen this point, and to have corrected Watson's error by speaking throughout in 'molar' terms.

Once this point is cleared up, the facts and theories of molecular behaviorism ('muscle-twitch psychology') return where they belong, to the province of physiology. Traditionally, bodily movements have always been the concern of the physiologist; the effects of bodily movements have been the concern of the psychologist. There seems to be no reason to alter this division of labor. It is not denied that the close co-operation of the two sciences may be highly profitable, but logically they are distinct. Bodily movements are simply the organism's shifts in space, and are not the major concern

of many psychologists. If, of the many contexts to which the organism belongs, we are interested only in the spatial, we can only answer the question, "What is the man doing?" in terms such as "His arm is moving six inches to the left, his feet are describing circles of ten inches diameter, his trunk is supported on his hands at an angle of seventy-five degrees," and so on. We cannot even say, "He is performing an acrobatic stunt," or "He is walking towards the door." Descriptions such as these include more than (physical) spatial relations. They place the shifts in spatial position in a larger context. A man may make the same movements of his limbs in walking to the door as he does in walking past the window. The 'walking,' considered as nothing more than certain movements of his limbs in space, is the same in both cases.

The reduction of all behavior to muscle-twitches has been attacked by many psychologists, notably the Gestaltists and Tolman. Many of them, however, have failed to rid themselves completely of an unreasoning reverence for the physiologist. Their failure is indicated by their belief that eventually psychological explanations must give way to physiological ones. This view is the last stronghold of the notion that by reducing molar behavior to its bodily movements, we can explain it. It is even found, in disguised form, in Koffka (4). It is disguised insofar as he holds that the ultimate physiological explanation will be in terms of 'nervous fields' rather than in terms of reflex arcs. It is, then, a molar neurological explanation to which he looks forward. But when we have this molar neurology, the foundations of which are already being laid by the work of Lashley, we shall have merely a more complete account of bodily movements. We shall see them in relation to one another, and shall be able to explain the pattern of bodily movements as a whole. We shall not, however, have an explanation of such behavior as earning a living, realizing an ambition, and so on. No neurology can ever give an account of these behaviors, for they consist of relations *between* bodily movements *per se* and other variables.

To recapitulate: The organism is immersed at any given instant in a number of contexts, or fields. One such context is the geographical environment described in terms of its purely physical properties. Behavior in this context consists of bodily movements in space, *per se*. While the organism is shifting its position in space, however, it is also shifting its position in a number of other contexts,[2] each of which is simply the geographical environment described in terms of its significance for the organism in some particular respect, such as its food-supplying facilities, or its implications for self-preservation, and so on. The events in a particular context cannot be explained by reducing them formally to events in another context. The controversy between Watson and the dynamicists arose from a failure to recognize this, and the belief that psychological explanations will eventually be replaced by explanations in physiological terms also harbors this error.

There are other examples of sterile psychological controversy which spring from the misconception that events in one universe of discourse can be explained by reducing them to events in another. One such point of issue is the Behaviorist account of neurotic symptoms in terms of conditioned responses. Watson's proposed method of psychotherapy (10) which is simply re-education of the undesirable habits, would be viewed sceptically by the dynamicists, who, it must be admitted, have made this field their own. They would point out that by gradual training, a patient may be cured of almost any symptom, only to develop another one soon afterwards. For permanent cures, not only the symptoms but the conflicts underlying them must be treated. Here the evidence is undoubtedly in favor of the dynamicists, and yet the explanation in terms of conditioned responses has its value. It depends which questions we ask. If we ask why a certain patient has a phobia of heights, the answer will almost surely be in terms of certain specific experiences of his past.

[2] It is even possible for the organism to shift its position in other contexts without shifting its spatial position. If a teacher orders a pupil to stand up, and he remains seated, making no movement, he is behaving (or misbehaving) nevertheless!

On the other hand, if we ask why this patient has a neurotic symptom at all, the answer will be in terms of his mental conflicts. The symptom is different behavior according to the context to which it is related.

Another example is James's classic statement of his theory of emotion, "Commonsense says, we lose our fortune, are sorry and weep; we meet a bear, are frightened and run . . . the more rational statement is that we feel sorry because we cry; angry because we strike, afraid because we tremble" (3, p. 450). Neither way of putting it is strictly admissible quite apart from the point James is making. The running is not the being afraid, nor is the being afraid the running. In the one case we are relating ourselves to the geographical environment; our behavior is then described as 'running.' In the other case, we are relating ourselves to the survival-implications of the geographical environment; our behavior is then described as 'being afraid.' Now it may well be that in fact, if no running occurs, no being afraid occurs, and that whenever running occurs, being afraid occurs. If this can be demonstrated, we can legitimately link events in one context in a causal sequence with events in another. This is not the same as a mere *formal reduction* of events in the one universe of discourse to events in another. That is the cardinal error. It may often be very valuable to know whether events in different universes of discourse are related in any way, but any propositions about such relationships must be capable of empirical demonstration, and not mere formal truisms.

Two basic problems remain. The first is to decide in which contexts the organism should be studied; the second is to decide what concepts shall be employed to formulate events in these contexts.

The answer to the first question will vary according to our purpose. A given bodily movement presents a variety of psychological problems, and according to his interests at the moment, the psychologist may choose to study any of a number of possible contexts in which the organism is set. There is no reason to call any of these contexts more basic,

or more fundamental, than any other; for some problems one will be important, in other problems another context may be the significant one. For example, it might be quite significant, for the study of emotion, to classify the organism's bodily movements in terms of their function in maintaining physiological states. (In Cannon's terminology, the organism would be said to be preserving its homeostasis.) If, however, one is concerned to discover the extent to which the forms of social behavior which are displayed in our society are innate, and one proceeds to examine various human societies to see whether our forms of social behavior are universal, a classification of bodily movements in terms of their homeostatic function is not of much use. What is required is a much more detailed classification of bodily movements, in terms of their effect in furthering various interests, such as the competitive interest, the acquisitive interest, the constructive interest, and so on.

This answer amounts to saying that there is no point in drawing up a list of 'basic drives,'[3] for this problem is identical with that of deciding in which context the organism is to be studied. To say that the organism has a hunger drive is only to say that certain of its bodily movements may be regarded as working towards the ingestion of food, that is to say, that at times the organism may be set in a certain context. Drawing up a list of basic drives is therefore, no different from drawing up a list of the basic contexts in which the organism is to be studied. It is true that one could indicate which contexts are most commonly used by psychologists, but there seems little point in doing this. It is better to take the problems as they come, and choose our context to suit the problem, rather than neglect problems because our context does not direct our attention to them. Which context is most useful for a particular problem of psychology can only be determined by the relative merits of the explanations offered by investigators using different contexts. It might be argued, for instance, that Watson's view of 'emotional' behavior as consisting of

[3] It is emphasized that objection is not taken to the concept of 'drive,' as such, but only to the tendency to rank drives as more or less fundamental.

bodily movements *per se* is not satisfactory, since it leaves unexplained some of the most important characteristics of 'emotion.'

The search for basic drives is carried to its extreme in those theories of motivation which regard behavior as essentially tending towards the maintenance of some condition of equilibrium. The equilibrium-state may be described in physiological terms, such as those designated as 'homeostasis' by Cannon, or as 'physiological invariability' by Rignano. Or it may not be conceived as a physiological state, but rather as a conceptual tool which may be employed in formulating the occurrences we call behavior. The organism is then said to be in equilibrium when it exhibits no tendency to change its relationship with the environment. This usage is represented by Koffka and Lewin, among others. Neither concept of equilibrium avoids the error involved in all attempts to reduce behavior to the manifestation of a few 'basic' drives. The important point is not the fact that all behavior tends to preserve 'homeostasis,' but the relationships between organism and environment which are requisite to the maintenance of 'homeostasis.' The important question is, Why does this organism strive to maintain its 'homeostasis' in these particular ways? If on the other hand the equilibrium state is not conceived as a physiological state, but merely as a condition in which the organism manifests no tendency to change its relationship with the environment, the important question is, What kinds of relationship with the environment are equilibrium situations? In neither case does the statement, that all behavior tends to maintain a state of equilibrium, explain or even direct our attention to the problems requiring explanation.

The second problem, *viz.*, what concepts shall be employed to formulate events in the various psychological contexts, is more difficult, and at present can only be given a negative answer. Our present concepts of motivation all err in that they locate the forces determining behavior *in* the organism. We tend to think of drives, needs (or what you will), as forces that push and pull the organism about. In reality, all we

have are observed uniformities in the sequence of events in a particular context. We observe that whenever an organism remains without food for a number of hours, it begins to alter its relationship with the environment, and continues to do so until food is ingested. When we say that "this organism has a hunger drive," we are simply giving a *name* to the uniformity of sequence of events which we have observed. There is nothing mysterious about it, nor is there any need to hypothecate the existence of forces within the organism which push the organism towards the ingestion of food. Only the unsophisticated use the concept of 'force' in physics as anything more than a metaphor. 'Force' in physics is a name for a particular relationship of two variables, mass and acceleration. If a physicist should be so far carried away by the animistic language he uses as to say that iron filings move towards a magnet *because* an electro-magnetic force attracts (or moves) them, he has only to phrase his statement in mathematical terms to see its tautological character. Psychology (as yet) has no such safeguard, and so we go on giving play to our animistic inclinations, and our notions of human grandeur, by placing the forces determining behavior in ourselves.

The very word which has come to stand for the problem of explaining behavior, 'motivation,' betrays this bias. The theory of motivation began as a theory of human motives or drives and impulses to action. But the theory has outgrown its name. The problem of motivation is taken to mean the problem of explaining behavior,[4] and a motive is often thought of as anything which moves the organism: that is to say, anything that affects behavior. If by 'motive' is meant a force located within the organism, the term 'motivation' is no longer an appropriate designation of the problem of the explanation of behavior. We do not think of the forces determining the behavior of the iron filings as being located *in* the filings; why should we be unable to conceive of human beings being moved by forces that do not reside within us,

[4] This usage was firmly established by Troland (9).

but are merely names for observed uniformities of actions in response to a given stimulus situation in a particular context?

To have seen this problem and to have attempted to work out a conceptual language free from this error, is the major contribution of Lewin (**5**). His conception of 'psychological forces' does not place them within the organism or in the environment, but in the dynamic (mutually dependent) interaction of the two. It is difficult as yet to evaluate the merit of Lewin's efforts in this direction, but certainly he has gone further than anyone else along the road to a satisfactory conceptual formulation of behavior.

In the co-operative volume, *Explorations in Personality*, Murray (**6**) and his collaborators recognize the same problem, and attempt to overcome it by a classification of the environment. They point out that in the past a group of stimuli '*A*' has been said to evoke a group of responses '*B*,' without any enquiry into what the various members of group '*A*' or group '*B*' have in common. Too often, the only thing that the group of stimuli '*A*' have in common is that they all evoke the group of responses '*B*,' while the only feature common to all the responses of group '*B*' is that they are all evoked by the group of stimuli '*A*.' To avoid this tautology, Murray and his collaborators set to work to classify the environment in terms of 'press,' the significance of the situation for the welfare of the person. The 'press' of the environment is thus what it promises to do to the subject. The term 'thema' is employed by these writers to cover the interaction of person and environment which is behavior. A given 'thema' is made up of an active 'need' on the one hand and a 'press' on the other; the interaction of the two is responsible for behavior.

Such a conceptual scheme goes some way towards breaking down the distinction between the organism and the environment and conceiving of behavior as a single event occurring in a context consisting of the person and his environment in a dynamic (mutually dependent) relation. But it breaks down because the concepts of 'need' and 'press' are not independent. In some cases one cannot determine the

'press' of a situation without knowledge of the person's 'needs.' An examination failure, for instance, is a 'press' of lowered self-esteem to the student whose self-esteem rests largely on academic success. To the college tennis champion, however, who cares nothing for academic success, it may be a neutral 'press.' If the 'press' of the environment is relative to the 'need' of the organism, and 'need' is defined as a force within the organism (Murray puts it in the brain field), we are back to an organism being pushed and pulled about by internal forces, finding some environmental circumstances favorable, and others unfavorable, to the satisfaction of its inner drives.

What is required is concept which will locate the forces responsible for behavior unequivocally in the field, or context, in which the organism is set, and a conceptual language that will enable us to conceptualize shifts in position in a field. The older, commonsense theories locate the forces which determine behavior in the organism rather than in the organism-environment field. What is required are concepts which will picture the organism and its environment united in a single event, and which will formulate the shifts of the organism's position in whatever context it is studied in by the psychologist.

Summary

In the past much sterile controversy has risen from psychologists' failure to agree upon what are the data to be explained by psychology. The two most widely accepted conceptions of behavior have been (1) the organism's bodily movements, and (2) the effects of these movements.

In reality, behavior consists of changes in the relationships between the organism and a system of variables we call the environment. The organism is related to a number of environments, or contexts, at the one time, and there is no limit to the number of contexts that may be distinguished. Where psychologists have often erred, is in attempting to explain events in one such context by reducing them formally to events in another context. Such attempts add nothing to

our understanding of behavior and tend to arouse useless controversy. A last stronghold of the error is the belief that eventually all psychological explanations will give way to physiological ones. Physiology may explain bodily movements, but it can never explain the relations between bodily movements and other variables.

The above conclusions have two important implications for the theory of motivation. They imply first, that there is no point in drawing up a list of 'basic drives,' since this is the same as making a list of basic contexts in which the organism is to be studied, and different contexts will be important in the solution of different problems. No context is 'basic' to any other in the sense that events in the second can be reduced to events in the first.

Secondly, the above conclusions imply that, with the exception of Lewin's concepts, current theories of motivation are misleading because concepts such as motive, desire, drive, need, wish, etc., are not satisfactory from this point of view (it is to be regretted that even Lewin has been driven to use the term 'need,' though in his later works he uses 'force' and 'vector'). We may be driven to coin a term, such as *envorg*, if no satisfactory term is available from physics or some other science. The difficulty with a neologism is that it removes psychology still further from the world of everyday life, and if we are to justify our doing this by the example of physics, we must live up to the example and produce equally practical results. At all events, the basic problem for all psychologists who are concerned to give a satisfactory explanation of behavior is to develop concepts which will picture the organism and the environment united in a single event, and which will formulate the shifts in the organism's position in whatever context it is studied by the psychologist.

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THE ROLE OF FRUSTRATION IN SOCIAL MOVEMENTS [1]

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I. The Difference Between Frustration and Motivation

The determiners of action are usually considered to reside in the psychological processes which are treated under the topic of motivation. Why we do one thing rather than another is considered to be a matter of the relative strengths of existing needs and the incentives which satisfy them. Group behavior is explained in terms of common interests which in turn can be reduced to common needs. Since needs vary in intensity, the study of motivation assumes that the measurements of needs and the satisfying properties of incentives hold the key to our understanding of the springs of human action.

Without question much of individual behavior must be considered from this point of view. That there are no qualitative differences among the possible types of determiners of action, however, remains a possibility which warrants examination.

Studies of frustration reveal that aggressive behavior is a frequent if not inevitable consequence. The case for the relationship between frustration and aggression has been thoroughly examined by Dollard, Doob, Miller, Mowrer, and Sears (2). They point out that a goal response which has been blocked gives rise to aggressive behavior. The aggression may be directed at the frustrating agent, or even at innocent bystanders. Its form shows a great variation so that it is difficult to predict the nature of the aggressive acts.

[1] Professor T. M. Newcomb has given me valuable criticism and stimulation in the development of this study. Students in my graduate seminar have also contributed their reactions. For this aid I wish to acknowledge my indebtedness.

Although frustration implies that a 'need' satisfaction has suffered interference, it is my contention that once the frustration has occurred, the previous goal may cease to determine the behavior. Aggression then becomes a function of the frustration, the previously existing goal response having been replaced by behavior which is controlled by an entirely different process.

Regressive behavior is also shown to result from frustration. This position is admirably set forth by Barker, Dembo, and Lewin (1). Their experiments showed that after frustration the play of nursery school children becomes more like that of younger children. On the average the constructiveness of play activity regressed nearly one and one-half years. The amount of regression was also found to be related to the degree of frustration experienced by the individual child. Aggressive and escape activity were also present, but these were independent of the regressive behavior.

Several studies have reported that fixations or stereotyped forms of behavior arise as a consequence of frustration. Maier, Glaser, and Klee (5), and Kleemeier (4) have particularly emphasized this behavior property in animals placed in frustrating situations, and Hamilton (3) and Patrick (6) have shown that stereotypy appears in the behavior of individuals subjected to emotional states.

Aggression, regression, and fixation are recognized forms of behavior closely related to frustration. Their form is not determined by apparent needs or incentives which ordinarily determine *what* behavior will come to expression. In motivated behavior the response makes possible the satisfaction of certain existing needs or desires. The above types of behavior satisfy no needs unless frustration can be regarded as a producer of needs. To treat frustration as a kind of motive in behavior, however, may represent a distortion of our concept of needs and explain nothing. The alternative is to grant that frustration is a qualitatively different determiner of behavior.

Although it might be feasible to attempt to reduce frustration to motivation or vice versa, it is well to examine the two

processes and see whether they are different in some basic manner. Any basic difference would preclude an attempt to reduce them to a single dimension since the units of measurements would have to be different.

In some discrimination experiments Maier, Glaser and Klee (5) found that accumulated frustration produced position habits in rats. Such animals fail to choose a member of a pair of cards on the basis of the appearance of the cards, but choose the member of the pair which is in the right or left position. Since position habits can also be established by rewarding the animal for the positional response (motivation), it is possible to compare the same responses established under the two conditions. It was found that the position habits established under the two conditions differed fundamentally in that those formed by frustration were subject to modification only when the animal was prevented from reacting, whereas those formed through motivation could be changed by merely altering the method of reward. Under the first condition many animals were unable to learn a new response whereas under the second condition the old habit was readily replaced by a new and more adaptive response. The two position habits which superficially were identical showed themselves to be qualitatively different as soon as attempts were made to alter them. As a matter of fact it was shown that animals persisted in their fixations even after they had learned that they were wrong. This persistence was shown by punishing the animals whenever they chose a certain member of the pair of cards. Since this punishment card was sometimes on the position side, the animals on these occasions were punished for choosing on the basis of position. Hundreds of experiences of punishment did not alter the choice based on position, but it did alter the manner of choosing. Whenever the punishment card was on the position side, the animals hesitated and set themselves for the punishment, but they did not do either of these when the neutral card was on the position side. This difference in behavior shows that the cards were actually differentiated, but the animals were unable to use this learning because their

position responses stood in the way. By forcibly restraining the animals' position response it was possible to produce a response to the opposite position. When this was accomplished the position habit disappeared and the animals were able to choose the nonpunishment card perfectly. The fixation may, therefore, be regarded as a compulsion because it is not altered by later experiences which conflict with it and lead to punishment.

Kleemeier obtained similar though less striking results by giving the rat an electric shock at the point of choice of a simple maze. The shock caused the animals to run into any one of four compartments, and the compartment chosen was more strongly preferred thereafter than preference established by repeated reward.

The above experiments show that the same habits formed under conditions of frustration and motivation have basically different properties. If motivation and frustration involved the same psychological mechanisms, we should be able to describe their functions by the same laws. The evidence, although limited, seems to justify making the hypothesis that frustration and motivation are qualitatively different instigators of action. The pragmatic value of this hypothesis must, in the end, depend upon its effectiveness in analyzing behavior which remains unclear when this assumption is not made. Having made the distinction between goal-motivated and frustration-instigated behavior, I shall now feel free to utilize all of the specific behavior symptoms associated with frustration whenever analyzing behavior which is initiated by it.

II. The Two Types of Social Movement

The qualitative distinction made in the above discussion already suggests that attitudes formed under conditions of frustration will not be changed by the same conditions which change attitudes formed by motivated learning. The former attitudes will appear stubborn and unadaptive since, from rational considerations, we expect attitudes to change when they conflict with larger patterns of experience.

Although stereotyped attitudes are important in group behavior, the other effects of frustration are perhaps even more important to social psychology. We know that frustration frequently produces aggression and that the aggressive behavior tends to be directed at the perceived frustrating agent. Aggressive behavior is also destructive in nature.

Suppose we attempt to induce a child to shovel snow. If he resists, we shall threaten him with punishment. The child is now pushed into the task by the threat and pushed from it by the unpleasantness of the task. This conflict may be regarded either as a conflict between motives or as a frustrating situation. If the child tends to attack the parent, he shows evidence of frustration. If he now withholds the aggressive response because the punishment may thereby be increased, his frustration is further intensified. Tensions now accumulate. In this condition the child will be very suggestible. Another child may propose that they run away together. Here then is a way out and the tensions find their release in escape activity. The other child may also increase the frustration by asking him to play ball or by poking fun at him. The behavior of the frustrated child in either case will depend upon the accumulated strains already set up. Whatever he does must be interpreted in the light of his existing tensions which have had no avenue for expression.

Let us now consider the larger problem of group behavior. The German people experienced a long period of frustration which was heightened by the depression. This fact, I believe, is sufficiently obvious not to demand further elucidation. One aspect of Hitler's influence was further to increase the frustration by reminding them of their lost honor and the brutality of the Versailles treaty. He even introduced frustrating factors when he subjected the people to personal sacrifices, to the interference of the Gestapo, etc.

This mass frustration in Germany had no immediate outlet. Aggression was excluded because the cause was either too vague or too powerful, and the tensions accumulated. With these tensions crying for relief Hitler now organized or channelized the aggression. Frustration does not

demand a specific act. Any form of aggression is a possibility, but a strong social movement requires unity. By pointing his finger at Jews, Communism, Imperialism, etc., Hitler focalized the aggressive action to meet the demands of the time. Frustrated individuals are easily organized and unified, although frustration itself contains none of the properties making for unification. Rather this unity must be imposed from without and leadership is, therefore, essential. By synchronizing the aggression the leader can give the movement its force.

The movement, however, is not only strong because of the united action of the individuals. Each individual gains further strength and confidence because of common action. Doubts or alternative acts seldom come to the individual's attention, so choice behavior is at a minimum.

The tensions produced under frustration have many aspects which make them appear abnormal. Unless they are released in immediate aggression they are pent up and have no direction in themselves. Any behavior which comes to expression, therefore, will be without motive and hence will appear senseless. Since aggressive behavior is destructive in nature, movements which are built up around aggression are characterized by destruction.

Both because of the amount of energy and the unity in its release, movements of this sort develop abnormal strength and the behavior of its members is difficult to understand by individuals not so organized. Frustrated individuals being irrational and regressive in their emotions, it follows that aggression-controlled movements will contain a high percentage of fanatics. Those of unstable make-up may actually be insane and still maintain a position in the group.

The psychological structure of social organization based upon frustration and aggression is necessarily primitive in nature. The group is organized around a leader who supplies the unifying stimulus. It is he who determines the nature of the aggressive pattern. The aggressive actions of part of the individuals in the group may influence those of others. Here imitation may play a part and exert an added

unifying factor. Basically, however, such social organization stimulates a swarm of bees which is held together by the common reaction to the olfactory stimulus supplied by the queen bee.

Because frustration produces regression, the change from complex to primitive social organization is readily accomplished. Mob behavior illustrates this very well. As long as the aggression is in progress the group is held together. When the aggression ceases, the group disintegrates.

In contrast, groups organized around a common goal will move toward that goal and away from alternative goals only insofar as the projected goal is attractive. Its attractiveness will vary in different individuals and for some whose vision is limited it will not exist at all.

To make a social movement, a large group of individuals must experience the same needs and demand the same goals as satisfiers of the needs. Further, these experiences must coincide in time. Propaganda at best may co-ordinate the needs. Only experience can greatly influence their acquisition. A large and forceful movement, therefore, is not a very strong possibility. It is more probable that competitive movements will arise.

Goal motivated social movements, when they do arise, will thus be limited in the number of followers. Since it is not common frustration which brings the individuals together, the movement is less likely to contain fanatics. Because goal motivation is associated with high performance in problem situations, the group behavior will be relatively rational and constructive. Obstacles standing in the way of the goal will be approached with the view of surmounting and passing beyond them. The goal being the determining factor, the behavior will always function in a directive and consistent fashion.

The psychological structure of social organization based upon motivation tends to be complex in nature because of the varied pattern of goals existing in different individuals. People have many needs the relative strengths of which vary from person to person. Frequently choices are involved and

in this matter individuals differ greatly. What is the most desirable choice is necessarily a personal matter, and to create harmony each member of a group must take the others into consideration. This group interaction is a necessary aspect of the social structure and is more complex than group interaction which is primarily imitative. The patterning of these varied intergroup-relationships gives the whole organization its complex character. Only in the higher animal forms do we find one individual taking the needs of another into consideration. Social organization which incorporates this property is obviously of a higher type than one which does not.

As the structure of the two types of social movements is different, the functions of the leaders also contrast with each other. In frustration-aggression controlled movements, the leader directs and releases the energy built up through frustration. He is at the throttle and can manipulate the energy. Being in this position, he can determine the course of history. To the extent that such leaders have existed in history, it can be said that personality is a determiner of history.

In groups organized around goals, the leader serves largely as a representative of the group. Instead of primarily directing the group's goal motivations, he, to a greater degree, is directed by them. Consequently the structure of the group greatly determines what the nature of the leader will be. The leader in power is more a product of the social order than a determiner of it. However, he can help them solve the group's problems, formulate plans for adoption and serve as a rallying point for people with similar goals. If he performs these duties well, he serves as an expert adviser and representative and in this way makes his important contribution to the group.

In order that the first type of leader be successful, he must direct aggression in a successful manner. If the aggressive action leads to frustration, he may not only lose power, but the new frustrations may be directed against him. To remain in power, he must continue to direct successful aggression since the alternative is disorganization. In a state of disor-

ganization revolution or a redirected aggression from another source is a strong possibility.

The second type of leader is successful as long as he represents the group. If his actions conflict with the majority, he is displaced but not destroyed. If such displacement is made impossible then he serves as a frustrating agent and alters the constitution of his group. Factions develop and new subgroups will be formed, some of which will be organized around frustration. The successful leader of this second type must, therefore, remain in close contact with his people. His observance of public opinion polls will reflect how accurately he perceives his role as a leader.

The responsibility of the two leaders also differ. The first type of leader must avoid blame. Errors he makes must be covered. A free press, therefore, is out of the question. The second type of leader may survive his errors and he may even admit them. He can protect himself by the pronoun, 'we.' A free press is an aid to him in that it reflects the needs of his group.

If Fascism and Democracy represent relatively pure cases of the two types of social movement, it follows that the goal for 'the new order' is not essential to the strength of the Nazi movement, although a similar term would serve the democracies as long as they remain primarily goal motivated. When Hitler makes reference to 'the new order,' the term is used to create further frustration in the sense that England or the United States stand in the way. The goal as such is very limited in function and appears more important in the foreign than in the local scene.

III. Practical Implications

The above analysis has certain implications which appear necessarily to follow. Since the accuracy of an analysis may be judged by the correctness of its implications, it is advisable to present them. Many of them can be tested and these may serve to support or refute the analysis.

1. No great or effective movement is possible unless it rests primarily upon previous frustration. Suffering thus

seems to be a prerequisite to reform. Leadership can achieve its greatest heights as well as its greatest depths when it organizes frustrated individuals.

2. Socialism stresses the goal whereas Communism stresses the destruction of capital. It seems that these two movements are psychologically different despite political and economic similarities. Socialism seems to be a movement organized around a goal and it has been a movement without great vigor. Communism contains the feature of frustration and it has been a much more vital movement. It follows that an analysis of the membership would show the former to contain a high percentage of idealists and rational individuals, whereas the Communist membership would show a large percentage of frustrated individuals.

3. People are more easily organized around what they do not want than around what they do want. What people positively desire is always in the nature of a goal. What people do not desire may be a negative goal (or the less desirable of two alternatives) and hence the opposite of positive goals; but it may also be something which either has frustrated them, or represents a substitute aggressive possibility for frustrations produced in other manners. It is the aspect of frustration which gives people's dislikes a unifying character.

4. Nations are more effective in waging wars than in constructing a peace program. Waging war involves frustration whereas peace is a goal. After World War I, movements advocating peace were termed idealistic and impractical (a treatment often given goals) and it was impossible to develop a strong peace movement.

5. To be effective propaganda must take into account the nature of the group it will influence. Obviously goals and appeals to reason will not stop a movement built on aggression. The only solution to aggression, once it is under way, is one which blocks or frustrates the aggression and thus forces it to take a different course (*e.g.*, revolution). To do this planes must drop bombs rather than leaflets on civilian objectives. Propaganda however serves an important purpose in initiating and maintaining aggressive movements. Its char-

acter must serve to increase frustration and direct aggression. Logic is not essential.

In goal-motivated societies the propaganda is more of the educational type. It emphasizes the goals to be attained and may attempt to create needs. It will contain statements of facts and appear logical. German agents and sympathizers have effectively used this form of propaganda in other countries. It stands in contrast to the belligerent and irrational character of the propaganda used within Germany.

Bombs will not alter the goals, but they may produce frustration and replace the goals. The frustration must then be organized in an aggressive pattern directed at the enemy. Since the bombing is not the result of aggression, the principle of a redirection of energy does not apply. Appeasement has little propaganda value since it represents an attempt to treat a society built upon frustration as if it were goal motivated.

6. At the present time Germany is the greatest source of frustration for France. Ordinarily this would produce aggressive action against Germany, but such action is largely prevented by physical force. As a consequence only isolated and disorganized cases of aggression appear. This mass frustration represents a potential source of aggression which may be channelized. When the time comes German leadership can direct this against the Allied nations. The very brutality of Germany's treatment of France may, therefore, prepare the way for collaboration. Considerate treatment of France by the Allies will not prevent her from joining Germany's organized aggression.

The alternative lies in organizing France's aggression against Germany. An underground organization has little chance to form a mass movement so it must be done from without. An Allied invasion would aid in this, in fact almost any successful aggression against Germany would go a long way toward channelizing France's potential aggression.[2]

7. If the United States is to aid in the defeat of the Axis nations which are unified on the basis of frustration, and still

[2] This paper was written before Laval came to power.

retain its goal-motivated democratic way of life, it must be done with the counterbalance of superior physical equipment. Whether or not this can be done in the time available is a question which must be answered by those having knowledge of productive facilities. The increase of human efficiency in such production, however, is partially a psychological problem. Greater motivation, in the form of wages based on production, will increase individual performance and the formation of factory groups into competitive teams will increase co-operation. Government control of production facilities also seems desirable for unifying the goals. The goal for 'business as usual' conflicts with production for war.

8. The succession of defeats which have occurred and which may continue to occur for the United States will serve as sources of frustration. This frustration will prove disturbing and may alter the basic goals. Unless the resulting aggression is channelized, localized aggressive groups will readily form. Attacks on its allies, its leaders, labor, management, etc., therefore are to be expected. If these attacks become a threat to unity the only alternative is to capitalize on the aggressive tendencies by guiding them into aggression against the enemy. This fact, of course, means that propaganda which instills hate and advocates destruction of the enemy will be required. Frustration will then serve in the achievement of greater unity, and additional obstacles may be imposed purposely upon the people to further increase their frustration. Limitations of freedom, high taxes, and inconveniences of all sorts can have a unifying function if the aggression is controlled.

9. So that an aggressive pattern of great unity be achieved, the total population must become part of the same organization. Each individual must be given his part to play.

10. People must be convinced that particular individuals or groups are not profiting by the war. Special privileges are an internal source of frustration which will produce localized aggressive tendencies and divide rather than unify the people. To further reduce localized aggression, sources of jealousy must be reduced. One source of localized frustration

can be reduced if the rate of pay is based upon individual needs as well as type of work.

11. The introduction of hate for the enemy will naturally impose difficulties on the attainment of a wise peace settlement because peace terms should be goal motivated. The peace settlement will be a problem which our leaders will face and they will have to be trusted. Once the enemy is defeated the frustration will be relieved and there is a good probability that the situation will not be out of control. The risk is present, however, but it is not comparable to that of losing the war.

IV. Concluding Remarks

As long as we strive for goals we are farsighted and do not appreciate immediate difficulties. This attitude makes us naïve with reference to aggressors. We practice rational diplomacy in our dealings with potential enemies and speak of betrayal when the knife is plunged into our backs. We keep men with diverse and personal interests in public office and forgive them for their lack of foresight. Our farsighted interests see beyond the obstructions and it seems illogical and unjust that these very obstructions may destroy us. We feared Russia because we feared her goals more than we feared Japan and Germany for their frustrations. If we appreciate the nature of the social order against which we are waging war, we can avoid certain errors and protect ourselves from its propaganda. This recognition may make it possible to defeat the enemy without deteriorating to his level.

Students of social behavior did not anticipate the effectiveness of the Nazi movement. They did not believe in its force because it was not a constructive or logical program. If we want a free and happy life, then we must grant that goal-motivated behavior is far more desirable than behavior instigated by frustration. Unfortunately, however, what is good and what is powerful are not necessarily the same. Men should be organized around goals to achieve happy and normal lives, but it does not follow that men will become so organized. Our own social organization must be considered

in the light of the background of the rest of the world. Our failure to distinguish the desirable from the powerful properties of social organization has been our greatest source of weakness.

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SOME CRITICAL COMMENTS ON THEORY OF PERSONALITY

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I. Introduction

There are two major and persistent problems which confront one who is interested in a satisfactory theory of personality. The first problem is that of the nature and development of adequate *functional units* of personality. This is a problem of adequate analysis. The second problem is that of the nature and development of that total organization or integration which we summarily designate as personality. Often this second problem is designated as the *problem of unity*. This problem is one of adequate synthesis.

While we are interested in theory in general, we shall be chiefly concerned with the highly elaborated and refined theory of G. W. Allport. We may say at the outset that our criticisms are, on the whole, minor criticisms. It is as if we approached a good artist's statuary work and made minor changes in it, removing a piece here and adding a piece there. In the end, the main outline of the statue would remain basically the same.

II. Functional Units

At the outset any theory-maker is confronted with a temptation and a necessary choice. The units of personality, whatever they may be, may be in terms of muscular and glandular behavior or in terms of psycho-neural organizations which lie embedded in the organism and out of sight. The temptation, from the standpoint of direct quantitative measurement, is certainly to select units in terms of segments of behavior. If we adopt the view that personality is nothing more than the sum total of behavior-patterns, in the above-

mentioned sense, we are confining ourselves to observable and measurable phenomena. Even covert behavior can, with considerable success, be instrumentally observed and measured. These are very desirable things! If, on the other hand, we define personality in terms of the *inner man*, so to speak, these inner dispositions or psycho-neural organizations are hidden from sight. They cannot be observed; they can only be inferred! As Woodrow[1] has pointed out recently, there is always danger in dealing with 'unobservables.' Thus Woodrow says, "When, from measurements of behavior or its immediate results, one argues that he has measured some unobservable, conceived to exist in the individual, he is always on very unsafe ground." Again, "It is by no means my intention to deny the importance of what goes on in the individual. . . . It is desired simply to point out that the making of psychological measurements does not entail a penetration within the skins of our subjects, whether these be human beings or white rats; and that it should be possible, consequently, to formulate the relations existing between our measurements without making elaborate assumptions concerning the nature of the internal processes and their relations to our measured magnitudes."

If, however, we desire an explanatory psychology instead of a mere descriptive psychology—in other words, if we wish to have a causative psychology and not merely a correlational psychology—we are forced, we believe, to define personality in terms of inner systems or dispositions. Personality *in toto*, or any functional unit of personality, then lies behind muscular and glandular behavior. Personality, or any unit of personality, becomes an inferred phenomenon, subject to the limitations mentioned by Woodrow. Nevertheless, every science has its inferred 'reals,' its basic conceptual factors, which cannot be observed or directly measured!

Allport, coming out flatly for a definition of personality in terms of inferred inner psycho-neural organizations, has chosen 'trait' as his functional unit. He defines trait as "a

[1] H. Woodrow, The problem of general quantitative laws in psychology. *Psychol. Bull.*, 1942, 39, 1–27. Quotation from page 2.

generalized and focalized neuropsychic system"[2] within the individual. We may raise the question, at this point, as to whether or not it is wise to make the trait, as defined by Allport, the functional unit of personality, to the exclusion of other inner dispositions or 'determining tendencies.' Also, it may be pointed out that certain difficulties arise in the attempt to make clear the nature of the trait; at least it is next to impossible to mark it off from other dispositions.

Many names appear, both in popular speech and in the technical literature, for various and sundry inner psycho-neural systems, dispositions, or determining tendencies. Here are some, for example: sensation, perception, Aufgabe, attitude, idea, mental set, problem-in-mind, taste, interest, purpose, prejudice, trait, sentiment, belief, goal, desire, habit, and ideal. Others might be mentioned but this list will suffice for our purposes. Probably some of these names stand for the same psycho-neural disposition. But quite obviously some differ in some dimensions or respects.

Let us think, for the moment, in terms of these inner dispositions or organizations. At once one will think of two dimensions. First, there is the *temporal* or *durability dimension.* Some inner dispositions are relatively temporary while others are more durable. This dimension may be thought of as a linear continuum or scale, ranging from zero to the *n*th degree.

Likewise, one thinks at once of a second dimension of these inner systems, namely, the *specificity-generality dimension.* Again we may think of any psycho-neural disposition as lying somewhere on this, a second, linear continuum. Obviously, Allport's trait *stands far to the right on each of the two scales.* But how far to the right on each scale must the disposition lie to be called a trait? Here is where Allport encounters his greatest difficulty for he implies a clearness of definition of trait where nature has made no sharp division. As a matter of fact, according to Allport's treatment, the trait can scarcely emerge as a full-blown determining tendency until adulthood. Yet, since babyhood, these dispositions

[2] G. W. Allport, *Personality, a psychological interpretation,* 1937. P. 295.

have been in the process of developing in the two dimensions which we have just mentioned. Hence the question must arise, "What shall we do for adequate functional units during childhood and adolescence?"

We have, however, considered only *two* dimensions of these inner dispositions. We considered them first because they are the ones that Allport has emphasized in his criteria of trait. There may be other dimensions, such as the emotional dimension or the intellectual (G-factor) dimension.

Allport's treatment is masterly and his discriminations and classifications are finely drawn. He has shown an appreciation for the delicacy and complexity of personality that must evoke admiration. However, we feel that he has spun his theory to too fine a point! It seems to the writer much better to speak of a variety of determining tendencies or motivational factors, such as perceptions, ideas, purposes, habits, attitudes, and traits. In some matured adults perhaps the traits assume such overwhelming significance that the other degrees and patterns of dispositions become relatively insignificant—but only in those adults! Even in the adult, however, this trait-fruition is likely to fall far short of consummation! Only when an inner psycho-neural system has become developed and integrated to the degree that it stands far to the right side of the two linear continua of durability and generality, does it attain trait-status. And only when trait-status is attained can we regard lesser dispositions (less durable and less specific) as insignificant in personality!

We may well agree with Allport that his system of personality or trait-psychology gives the individual a tremendous degree of independence of the stimuli. But we must not go so far as to make any man *too independent* of the specific situation! Allport probably has no such intention but trait-psychology tends in that direction. We feel that we must still make considerable use of the relatively simple and humble little habits (not the 'generalized habits' of which Dewey speaks). Unless a person be completely and perfectly organized into 'trait-dispositions,' habits must enter into the motivational picture at times. Likewise, determining ten-

dencies of lesser degree of generality and of lesser degree of durability in the life-span must take their place side by side with the organizations which have attained sufficient dimensional status as to warrent their being called traits according to Allport.

III. Unity of Total Integration

Allport has probably done about all that anyone can do at the present time with the problem of total organization or unity in Chapter XIII of his book, *Personality*. We may agree with him as he concludes his chapter with these words, "The truth of the matter is that total organization of personality is still a new and poorly formulated problem in psychology. It is a many sided issue whose solution lies in the future." [3]

We take it for granted, first, that personality is a functional concept, and, second, that function depends upon structure. Allport himself defines personality in terms of 'Organization within the individual' of '*psychophysical systems*.' Trait is defined as a '*neuropsychic system*.' Obviously, then, Allport himself subscribes to the above propositions and realizes that personality rests upon the structure and functioning of the nervous system, with probable emphasis upon the cerebrum. However, realizing how very little we know about the physiological processes of the neurological structures, he makes the following statement, "That there are physiological correlates of integration involving complex spatial, temporal, histological, and electrochemical factors no one will deny. But since integration implies functional joining of nervous pathways, taking place presumably in the region of the synapse, and since this entire process lies still in the limbo of scientific mystery, all accounts of integrative growth in physiological terms are at the present time highly speculative." [4]

One may well agree with the above-mentioned statement. We may likewise express some sympathy with Allport when

[3] *Op. Cit.*, p. 365.
[4] *Op. Cit.*, p. 140–141.

he says that, "To escape from the deceptive snares of pseudo-physiology it will be safer to take from now on a frankly psychological approach to the whole course of growth, and here specifically to the problem of integration." [5]

The writer would merely hazard a few statements at this point. First, it is not clear as to just what a purely 'psychological approach' is to be. It is not so easy to differentiate between the 'purely psychological' and the 'purely physiological.' The writer would prefer to say that the psychological is merely an emphasis on certain apsects of the physiological, namely, the conscious and behavioral aspects.

Second, Allport must constantly find anchorage in the basic nervous organization and functioning, even though he fully realizes, as we all do, the extreme hypothetical nature of our knowledge thereof. The writer believes that the basic key to an understanding of the matter of integration of the smaller units, habits, attitudes, traits, or what you will, as well as the larger, total integration, must eventually be conceived in terms of the structure and function of the nervous system. Our understanding of total integration, organization, or unity will never be more than superficial until we know much more about those 'complex spatial, temporal, histological, and electrochemical factors' in the nervous system. We see no escape from this conclusion.

Third, if we adopt at present a strictly 'psychological approach,' we must remain on a descriptive level to a great degree. We, like Allport, want an explanatory and motivational system of personality. Function rests upon structure! Psychological functioning rests upon and is conditioned by nervous structure!

Let us, however, go along with Allport in his attempt to make a 'frankly psychological approach.' The writer has merely two suggestions to make here: (1) *let us think of personality as a multi-dimensional thing.* Let us proceed to regard unity as just one dimension. We may now think in terms of a uni-dimensional scale or linear continuum for some common aspect of this phenomenon of unity, still preserving Allport's

[5] *Op. Cit.*, p. 141.

emphasis upon individual uniqueness. (2) Let us, in the wisdom of our observations and needs, try to agree upon the criterion of unity. This last desideratum would be most difficult but well worth the attempt.

Allport has suggested 'consistency' as the outstanding criterion of unity or integration of personality. The writer likes the idea, if we can sufficiently clarify the concept and agree upon it. But whatever be the criterion, it must be a uni-dimensional thing. If we proceed to make unity a multi-dimensional thing, then we are left with a complex qualitative problem that is a nightmare to those who wish to measure and work out equations. So we would like to be able to think of unity in terms of a linear continuum, enabling us to speak of greater or less degrees of unity. The theoretical and practical advantages of this approach are fairly obvious. Allport would probably answer that any such uni-dimensional concept could deal only with a 'common aspect' of the individual unique modes of unity or organization. We would agree with this proposition.

Practice, as usual, runs far ahead of adequate theory. In psychiatry and abnormal psychology, such terms as abnormal, disorganization, and disintegration are used regularly. They imply some standard of unity or integration, which standard is usually pretty indefinite and vague. Obviously any attempt to work out linear continua for well-agreed-upon dimensions of personality would have positive practical and clinical, as well as theoretical, advantages.

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